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Institute of Marine Science

UNIVERSITY OF MIAMI

PROCEEDINGS OF THE 1965 ARMY CONFERENCE
ON TROPICAL METEOROLOGY

Miami Beach, Florida
6-7 May 1965

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Radar Meteorological Laboratory
INSTITUTE OF MARINE SCIENCE
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Miami, Florida 33149

PROCEEDINGS OF THE 1965 ARMY CONFERENCE ON TROPICAL METEOROLOGY

Held in

Miami Beach, Florida
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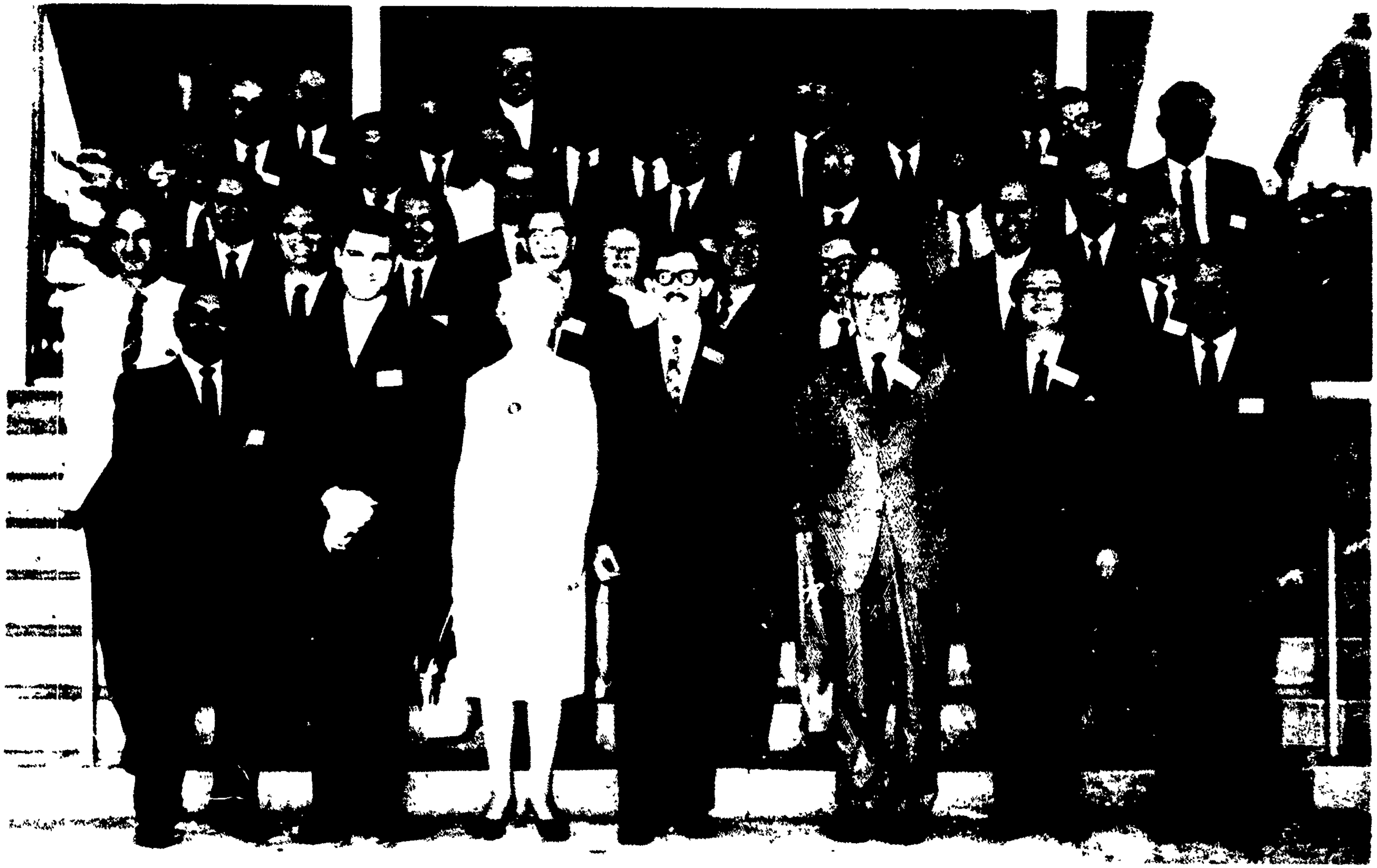
PREFACE

The 1965 Army Conference on Tropical Meteorology, the fourth consecutive annual review of tropical research sponsored by the U. S. Army Electronics Laboratories, was held in Miami Beach, Florida, 6-7 May 1965. Attendance was by invitation only and was limited to those who could be expected to actively contribute to the discussions. The interchange of ideas was extremely beneficial to all concerned.

These Proceedings include papers which have been submitted in final form by the speakers. The ensuing discussions which appear after each paper were transcribed from tape recordings made during the conference. Although certain liberties have been taken by the editors to edit the discernible remarks, every effort has been made to present a clear and concise summary that is essentially verbatim. Areas which were not discernible are indicated as such. Papers and remarks are arranged according to general content in agreement with the program of the meeting.



Hiser Martin Estoque Rosenthal Hawkins Lessmann Gentry

Kerr Bargman La Seur Diem

Freeman Frank Gerrish Erickson Garstang List Dunn Cry

Cobb Gulick Alaka Frisby

Kraus Kurtz Arnason Weickmann Goldman Kuhlthau Hubert

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BACKGROUND AND PURPOSE OF THE 1965 ARMY CONFERENCE ON TROPICAL METEOROLOGY

H. W. Hiser

This Conference on tropical meteorology is a successor to three similar meetings held in May 1962, '63 and '64 under sponsorship of the Atmospheric Physics Branch, U.S. Army Electronics Laboratories. The first Conference, in 1962, was essentially for the purpose of planning a broad program in tropical meteorology and reviewing the then state-of-the-art. The second and third Conferences, in 1963 and 1964, included research reports from the U.S. Army Contractors plus some very interesting and stimulating presentations by invited guests from the U.S. and abroad.

Although the primary interest of this meeting lies in the realm of local, small scale meteorological phenomena and problems, specifically excluding hurricanes and typhoons, we are not completely limited to these as can be seen from the agenda. We desire to learn as much as possible about the recent results and future plans of research by the U.S. Army Contractors and our other guests. We hope to ascertain what new problems have been encountered, especially those problems which need further study, and those in which there are widely different opinions as to solutions. We are particularly interested in having the benefit of the experience and special knowledge of our foreign visitors for comments and suggestions regarding these research activities and problems encountered.

A proceedings for the 1962 Conference was prepared by the Electronics Engineering Research Laboratory of the University of Texas and the proceedings for the 1963 Conference was prepared by the U.S. Army Electronics Laboratories. The proceedings of the 1964 Conference was prepared by the Radar Meteorological Laboratory, Institute of Marine Science, University of Miami, and the same arrangements have been made for this Conference. We request that all speakers furnish us copies of their papers by the close of this meeting. We will tape record the question and answer discussions during the meeting and try to include these following each paper in the proceedings. Copies of the proceedings will be made available to all participants.

LAKE LEVELS IN EAST AFRICA

D.J.Bargman, F.E.Lumb and H.T.Mörth
East African Meteorological Department

ABSTRACT

The profiles of Lakes Nyasa, Victoria and Tanganyika are examined in relation to the climate. For Lake Victoria, the best documented of the lakes, certain relationships are shown between rainfall, runoff, evaporation and outflow and the delicate balance is demonstrated. The extremely high rises in 1961 continuing to 1964 are described. Speculation on the causes, including climatic change to wetter conditions and a possible correlation with the 26 month upper wind reversal is offered.

INTRODUCTION

By the end of the first rainy season of 1964 the levels of all the major lakes in East Africa had reached heights never before recorded and far above previous systematically recorded maximum levels. Considerable damage has occurred to port installations, railways and roads, industrial and commercial plants (including water supply and electrical generating stations) as well as to housing. Not unnaturally the Governments of Tanzania, Kenya and Uganda are highly concerned of the present high levels being maintained or exceeded. Continuing increases in the level would have the most serious financial implications, particularly at some ten major lakeside towns.

LAKE NYASA

Records of lake levels commenced in 1899 but are of little value for study without a considerable amount of correction. Prior to 1916 the Lake possessed an outlet into the Shire River. Over the period 1916 to 1935 this outlet accreted and the outflow characteristics were completely changed resulting in a rise in the level. The lake profile since 1899 is shown in Fig. 1. A simple correction based on the assumption that the rise from 1916 to 1935 was linear gives a profile that is also shown in fig. 1.

LAKE TANGANYIKA

Records of lake levels are only available from 1923. Like Nyasa there is an outlet, the Lukinga river, this ran naturally until 1951 when a low weir was constructed. The profile of the lake since 1923 is shown in Fig. 2.

LAKE VICTORIA

Records of lake levels began in 1899 and have continued without break. As with the other two lakes there is an outlet, in this case at

Jinja into the Victoria Nile. It is the importance of this river to the downstream countries that has produced the good long term records of level. Until 1950 the river had a natural discharge at Ripon Falls, in 1950 construction of the Owen Falls Dam and Hydro-electric scheme commenced and natural flow as an annual mean was maintained during construction (there were temporary variations but these were corrected in a later month in the year). In 1954 the Dam was completed and the Hydro-electric scheme opened. Since that date the outflow has been maintained at the natural run of the river rate for the particular lake level. This is achieved by using an internationally agreed rating curve based on past records and there is little doubt that as long as the lake is below maximum levels recorded prior to 1950 the rating is accurate. For the present extremely high levels an extrapolation has to be made and there could be some possibility of the calculated "natural" discharge not being equal to the true natural discharge could it occur. The profile of Lake Victoria is shown in Fig. 3.

ALL THREE LAKES

Remembering that the catchments of these lakes extend, in order, Victoria, Tanganyika and Nyasa from 2° North to 15° South, nearly 1,200 miles, there are impressive coincidences of peakiness. The most marked coincidences are:-

1906/07	Lake Victoria and corrected Lake Nyasa
1917	Lake Victoria and corrected Lake Nyasa
1927	Lake Victoria and Tanganyika only. The assumption in correcting Lake Nyasa might mask this.
1932	All three lakes.
1937	All three; lasting over three years to 1940
1942	Lakes Tanganyika and Victoria only
1947	All three lakes
1952	All three lakes
1957	All three lakes
1962	All three with continuing coincident rises to 1964. The nature of these rises in all cases being far greater than at any time since records began at the end of the 19th century.

As most of the outlet values are known for these coincidental rises and, in fact, show themselves to be increases it is possible to eliminate a decreasing value of outflow as being the cause of the rises. Therefore the only parameters that are involved in the rise are increased direct rainfall, increased runoff, (itself a function of rainfall) or decreased evaporation. These are all part of climatic control and because of the coincidences this control is of, at least, sub-continental extent and could well be greater.

The other factor in the coincidences that attracts immediate attention is the apparent 5 year (or 10 year) periodicity.

The most startling fact in all the lake profiles and coincidences is the degree of the rise since the end of 1961.

HISTORY PRIOR TO 1899

No scientific records extend back before 1899. Reports from explorers in the mid 19th century tell of much higher lake levels than even the 1964 levels. Other evidence, ecological and geomorphological, also points to the fact that the lakes have been higher in the past.

Papers by Kraus (Q.J.Roy. Met. Soc. and the Proceedings of the UNESCO/WMO Symposium on Changes of Climate) postulate that in the mid 19th century tropical rainfall was higher than in the first half of the 20th century. (If this was so then tropical lakes could well have been higher). He suggests that the change to drier conditions in the tropics was world wide and occured about 1895. Other papers in the Proceedings on Changes of Climate indicate that there were certain climatic conditions in temperate latitudes during the mid 19th century, i.e. at the time when tropical rainfall has been postulated to have been high. These temperate latitude conditions also underwent a climatic change at the begining of the present century, i.e. at the time the tropical rainfall changed to a less wet regime.

Many authorities, well represented in the Proceedings on Climatic Change, have shown that there is a current change in the climate of temperate latitudes with a return to the conditions common a hundred years ago. If the previous relationship was not casual there could thus be a climatic change to wetter conditions commencing in the tropics.

1964 to ?

In speculating on the future of the lake levels there seem to be three climatic possibilities:-

(i) The climate has not changed and the 1961-63 rainfall was a very low probability variation.

(ii) There has been a step-like change where, after the heavy rains giving an abnormal runoff and interception, the climate has settled at a level where the new rainfall mean is only just above the mean of the past several years.

(iii) There is a continuing climatic change and mean rainfall is going to be higher than the past mean rainfall and will continue to become higher.

STATISTICAL ANALYSIS OF MONTHLY RAINFALL OVER LAKE VICTORIA CATCHMENT 1938-1964

The purpose of this analysis is to determine whether the heavy rains of 1961, 1962, 1963, and 1964 indicate a real change of rainfall regime which may continue for some years or whether it is reasonable to suppose that they have occurred by chance juxtaposition of individual years of the type which have occurred from time to time since the records of monthly average rainfall over the Lake Victoria catchment first became available in 1938.

Six-monthly totals (inches) of the monthly averages, commencing with each month of the year, are given below:

Jan. to June.	Feb. to July.	March to August	April to Sept.	May to Oct.	June to Nov.	July to Dec.	Aug. to Jan.	Sept. to Feb.	Oct. to Mar.	Nov. to Apr.	Dec. to May.
27.1	28.6	31.2	30.5	26.5	24.0	23.5	21.9	19.6	20.1	24.1	26.6

It is seen that the wettest six-monthly period is March-August (31.2"), and the dryest (19.6") commences exactly six months later, i.e. September-February. It therefore seemed appropriate to carry out the analysis separately for the wet half-year commencing in March and the dry half-year commencing in September.

The six-monthly totals commencing March and September for each year from 1938 to 1964 are set out in the table below:

Year	March to August		September to February of following year	
1938	30.9		17.0	
1939	26.0		18.5	
1940	33.5		16.6	
1941	32.0		24.1	
1942	38.2		13.2	
1943	27.6		12.0	
1944	29.7		22.3	
1945	27.9		14.6	
1946	35.5		24.4	
1947	34.4		15.9	
1948	30.4	Average	14.9	Average
1949	27.6	1938-1961	16.4	1938-1960
1950	32.2	30.7	15.9	18.3
1951	34.9		28.5	
1952	33.8		13.2	
1953	25.1		17.6	
1954	31.1		18.7	
1955	27.3		24.3	
1956	29.4		17.8	
1957	33.7		17.9	
1958	30.0		18.2	
1959	24.0		23.3	
1960	32.1		16.3	
1961	29.0		38.9	
1962	35.1	Average	26.5	Average
1963	33.7	1962-1964	25.4	1961-1963
1964	34.3*	34.4		30.3

* A first estimate of rainfall (5.42") based on Kenya stations only was used for August, 1964.

Considering first the rainfall data for March-August 1962, 1963, and 1964, all three six-monthly periods had above average rainfall.

However a wellknown statistical test (student's t-test) when applied to these data shows that the difference (3.7") between the average for three six-monthly periods (34.4") and the average for the previous 24 years (30.7") was just not significant at the 10% level. This means that the probability that these three above-average rainfalls for the March-August periods of 1962, 1963 and 1964 occurred by chance juxtaposition of three wet seasons of the type which occurred occasionally during the years 1938-1960 is as high as 1/10. We are therefore not justified in assuming that there has been any real change of rainfall regime for the period March-August as compared with the years 1938-1961. This result was to be expected, since there were other runs of 3 years with almost the same high rainfall during the months of March-August (1940-42 with mean 34.6" and 1950-52 with mean 33.6").

Turning to the rainfall data for September-February 1961-62, 1962-1963, and 1963-1964, all three six-monthly periods had rainfall very much in excess of the average, and the period September 1961 - February 1962 was outstanding (38.9" against an average of 19.6"). Student's t-test shows that the difference between the average for the 3 six-monthly periods (30.3") from the mean for the previous 23 years (18.3") is highly significant. The probability that these three consecutive above-average rainfalls for the September-February periods of 1961-1962, 1962-1963, 1963-1964 occurred by chance is about 1 in 1000. The statistical analysis therefore indicates that there has been a real and significant change of rainfall regime during the dry six-months September - February, but the evidence for a change of rainfall regime during the wet six-months March-August is by no means conclusive.

An examination of the September - February rainfalls in the above table shows that in every fifth year commencing with 1941 the six-monthly period September - February was very wet as compared with the neighbouring years. (The rainfalls for these wet periods have been underlined in the table. There is one exception to the rule. The high value of 1955 is one year too early).

Taking the years 1938-1960, the average rainfall of the four peak September - February periods is 25.33", and for the 19 other drier periods is 16.86", giving a ratio of the 4 peak rainfalls to the 19 others of almost exactly 3:2.

The extremely high value for September 1961 - February 1962 of 38.9 inches coincides with a peak of the 5-year cycle, and was followed by two six-month periods (September - February) with an average rainfall of 25.95. The ratio of 38.9 to 25.95 is also almost exactly 3:2. Therefore as a bold speculation we might suppose that a new pattern of rainfall, still based on a cycle of 5 years, has been established for the six months period September - February, i.e. a rainfall of about 39 inches in the peak year of the cycle, followed by four September - February periods with an average rainfall of (2/3 x 39) = 26 inches. On this assumption, the average rainfall for September - February will be

$$\frac{39 + (4x26)}{5} = 29",$$

and assuming no change to the March-August average rainfall (31"), the new average annual rainfall will be 60", as against an average for 1938-1960 of 49".

During the period 1938-1960, there was considerable variability of the rainfall for each September - February period during the non-peak years, about the mean value of 16.86". Assuming that under the new rainfall regime the percentage variability of the non-peak periods (September - February) in relation to the new peak rainfall (39") remains the same as for the years 1938-1960, it can be shown by well-known statistical methods that there is only one chance in 20 that the rainfall for any 6 months-periods September - February will be less than 21.3". Hence as soon as the figures for September 1964 - February 1965 are known, it will be possible to assess the probability that the new regime still prevails. In the meantime, in calculating the behaviour of the Lake over the next few years, it would be prudent to assume a new average rainfall of 60", as against the 1938-1960 average of 49".

FURTHER RESEARCH

Autocorrelation investigations into the catchment rainfall data have made good progress, and a full account will be given later. It appears at this stage that the 60-month cycle is statistically significant and that therefore a probability forecast of lake levels can be done on statistical grounds.

Research into co-variation of rainfall with other meteorological parameters is progressing, and the study of possible co-variation with extraterrestrial parameters has been included. Some relationship with the stratospheric wind reversal seems to be indicated from preliminary investigations.

SPECULATION

Analysis of the rainfall in the catchments of the other two lakes during the years 1961-64 show similar percentage increases. In view of the similarity of the peakiness it is reasonable to suppose that the same order of change will occur and for planning purposes a new mean rainfall of some 20% above the mean to 1961 should be used.

Further the possibility of extreme peaks in early 1967 must be kept well in mind unless the results of the autocorrelation show this to be statistically unsound. These high peaks would affect the lake shore economics but as well the generally higher rainfall would produce problems of wide spread flooding, road closures, crop destruction and famine.

Although this sounds depressing it must be remembered that an increase in the mean rainfall would bring vast areas of land with marginal rainfall into potential productivity. Water, even though it may damage, is still the most valuable mineral in East Africa; damage must be kept at a minimum by sound planning.

REFERENCES

Kraus. E.B.,	1955 a	Quart.J.R.Met.Soc.,81, p.198
	1955 b	Ibid.,81, p.430
	1956	Ibid.,82, p.96
UNESCO/WMO	1961	Changes of Climate

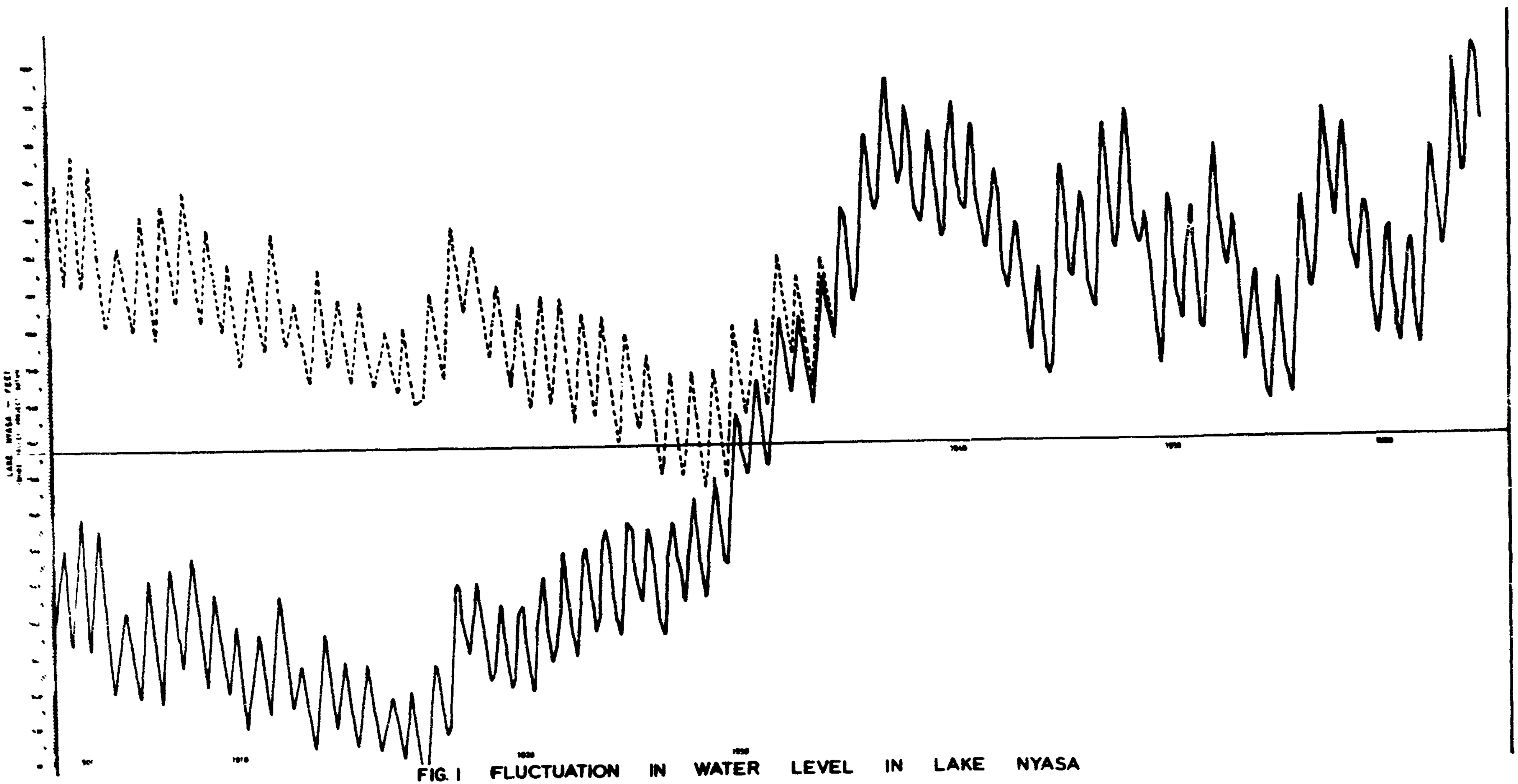

FIG. I FLUCTUATION IN WATER LEVEL IN LAKE NYASA

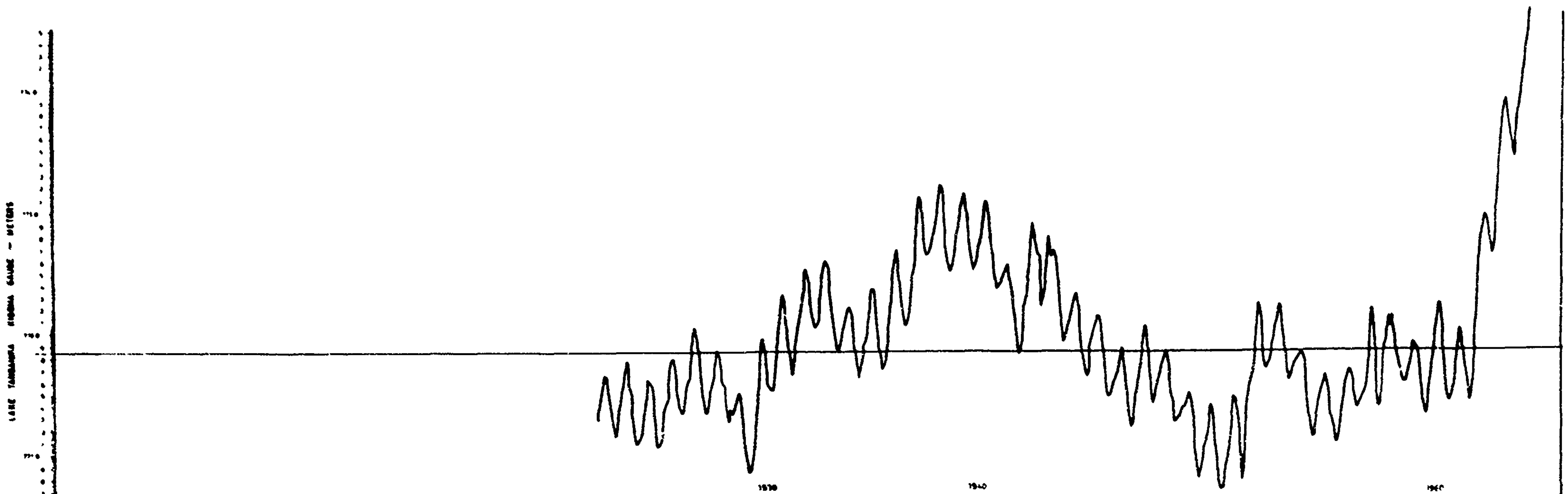

FIG. 2 FLUCTUATION IN WATER LEVEL IN LAKE TANGANYIKA

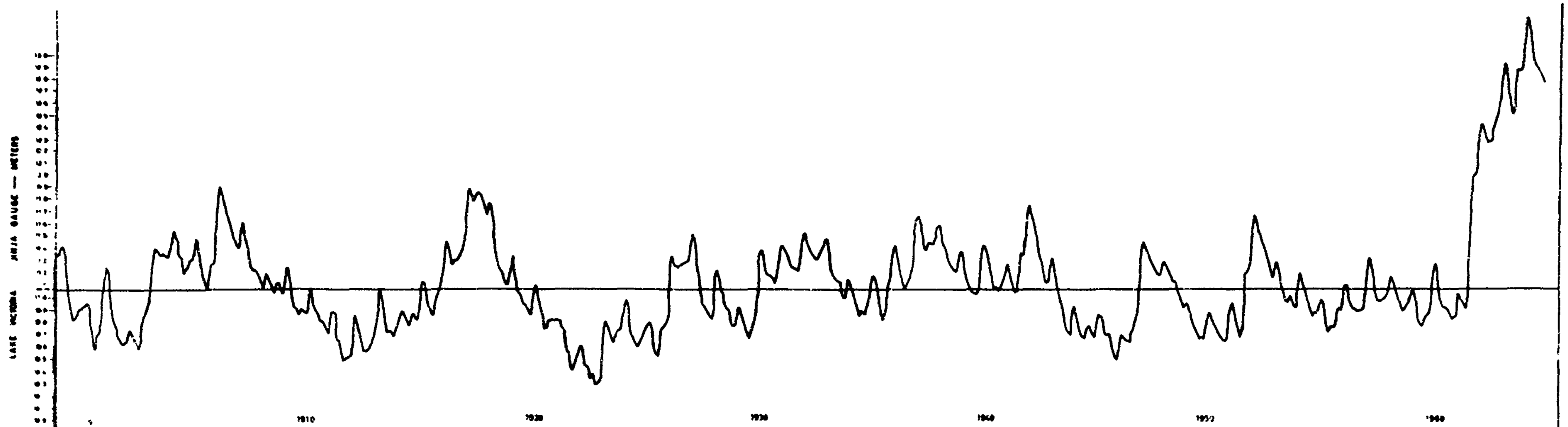

FIG. 3 FLUCTUATION IN WATER LEVEL IN LAKE VICTORIA

DISCUSSION ON BARGMAN'S PAPER

GRIFFITHS: You mentioned the average of the wettest and dryest six months, on how many stations is this based? I know that you have a very good network around Lake Victoria, but I am wondering whether this is still kept up during the past two or three years and whether it is representative, percentage-wise, of the higher territories and also of the rainfall over the Lake itself?

BARGMAN: The number of stations is better than 200. The distribution, of course, tends to weigh rather heavily around the inhabited areas. This tends to weight it perhaps in the areas where there is slightly higher rainfall than in other parts of the Catchment, and it can only be considered as representative of the rainfall interception by the Lake itself insofar as it does include the peripheral stations and the island stations on the Sese islands and the Ukerewe peninsulas. As to being completely representative of the rainfall in the center of the Lake, we just don't know, as you well realize.

GRIFFITHS: Concerning the run-off, by the time this gets down to Khartoum in confluence with the Blue Nile, etc., what difference does this make? I understand that most of this evaporation takes place in the Sud in the Southern Sudan, and I wondered percentage-wise how these compared now?

BARGMAN: All we can say is that the Sud at the moment from the 1961 period onward is flooding seriously and Southern Sudan is faced with considerable flood problems at the moment, to the degree that any control that could be exercised at the Owen Falls outlet in Lake Victoria, cannot be exercised because it would seriously embarrass the Sudanese government. This is one reason why the projected Lake Victoria hydrometeorological scheme, which is going forward through the UN special fund, is now being extended to invite participation by the United Arab Republic and the Sudan, as well as the three East African countries and to include Lakes Kyoga and Albert; so that the balance at the entrance to the Sud can be found and thus the Sudanese and the Egyptians from the flow at Khartoum can establish exactly what is happening to the control in the Sud.

KRAUS: Actually information on the flow of the Nile through East Africa during the last century is available; also the flow of the Nile in the 25 years or so since a control existed. The last one during the 19th Century was, I believe, about 28 or 25 per cent higher. It was a colossal amount of water, more than it was in the following 50 years. The change mainly occurred during the dry season. That is borne out by records from other places in the tropics which also have shown in this past work, which you kindly mentioned, that the dry season was the length of the rainy season, if you wish, in the way of crucial effect. I think you were an extremely brave man, and I think we all should be thankful that we were not in your shoes to forecast that this thing is going to last. It would be extremely difficult to be sure of that, and there are indications that the change which has occurred, but not exactly of the same dating, is again of a world-wide sort of pattern, and the rainfall, one of the things which has been found, that the rainfall in the tropics was quite strongly correlated on a long time basis with the total amount of rainfall which you found along the east coast of the continents up to higher latitudes. Indeed during the 1950's the average or accumulated rainfall along the east coast of the United states, and the east coast of Australia was again higher. I think there it depends upon what statistics you use, because I think if you would extend that up to 1964 in the United States, I doubt whether it would be higher. I don't know. It also has been found by Mitchell that, at least during the 1950's heating trends which seem to go into this in a higher latitude have come to an end. So there is some indication that there has been some change, but it certainly was striking to see this as dramatic and as large during the last few years in tropical lakes. Whether that is just an odd fluctuation or something that lasts, remains to be seen.

FREEMAN: I would like to suggest a further bold speculation - at least that you investigate it. And that is that you take an old record, like the records of the Nile, and your present records of the Nile, and compare it with the Krakatoa explosion and the recent dustcloud that we have in the stratosphere from whatever the name of the recent explosion is, and see if there is any correlation between the time of this increase of rainfall in the latter half of the nineteenth century and the dust in the stratosphere. There is definitely a measurable effect of this dust on, say, the amount of radiation that reaches the south pole. This has changed by more than 100%.

KRAUS: I think I can answer this last question. The effect of the Krakatoa investigation on the very high rainfall which has been recorded from the few stations which are available during the last century in the tropics has been investigated and no connection could be found from the data available in all tropical stations with long records.

BARGMAN: Dr. Morth who is doing the programming of the auto-correlations, is trying to include as many terrestrial, non-meteorological and extra-terrestrial phenomena as he can to see if anything does, in fact, come out. We are conscious that it may be connected with something similar to what Dr. Freeman has suggested. In this I would go on further with speculation - once one starts speculating, there is no end to it - there is, I think, quite a lot of thought going on at the moment as to whether vapor trails from supersonics are, in fact, going to cause persistance in increase of vapor in the very high levels from ice crystals coming from the contrails and whether this in turn is going to have any large-scale effect on the climate. It is tied up with such things as this that I think it is of interest.

KOTESWARAM: I was rather struck that 1917 was a year when you had a peak, and when I was thinking about the climatology of India, 1918 happened to be the worst year for the Indian monsoon. In fact, the Indian southwest monsoon taken as a whole runs out pretty smooth and it is very rarely that it fails, as they call it; but 1918 happened to be a sort of complete failure of the monsoon. I don't know how these two are correlated, but it is worth examining these two incidents. Number two, during the period 1942 to 1950, there was a downward slope. Now, that was the period when South India, which depends upon what is known as the northeast monsoon rainfall, that is the rainfall on the east coasts of the continents of the type which Dr. Kraus has mentioned, was experiencing the complete period of drought and it was only in 1953 that South India suddenly got an increase on the same day as mentioned for Nyasaland and Victoria. Such a thing was there in South India during the northeast monsoon period and these two have been correlated. I was wondering whether you have got any figures about South India and Ceylon during this period of 1960 to 1964, and whether there is any corresponding increase in the South Indian Northeast monsoon rainfall that might be quite interesting? Off-hand, I have not heard of any large increase that has been reported for India

BARGMAN: In fact, correspondence with our colleagues in your service seemed to indicate that certainly there has been no increase in the rainfall from the southwest monsoon from 1961 to 1963, but that if anything it might be a lower rainfall than normal. I believe some of your colleagues are interested in what we are doing and are going to work on the problems in India from the southwest monsoon and to see whether there is any relationship, whether we, in fact, are stealing your water before it gets to you.

KOTESWARAM: This I have no knowledge of, either from correspondents, or from talking with colleagues from the Indian Meteorological Service.

WEICKMANN: It would, of course, be very interesting of extending your studies to other great lakes, and I remember, for instance, that in this country the great lakes are in a comparatively low-level. Of course, not in meters, but in inches, but nevertheless, I think it would be extremely interesting. One lake I would be specifically interested in would be the Chad Lake in Northern Africa because the level of the Chad Lake has been known for, I think, many, many centuries, and is always appearing in problems of the pluvial periods and ice age periods. You may wish to make studies in connection with all the periods.

BARGMAN: Well, I have no quantitative knowledge of Lake Chad at the monent, but I was talking to a colleague who has been investigating Chad, only six weeks ago. Lake Chad is higher now and has more water in it than people can remember. There are serious lakeside flooding problems in the townships.

ESTOQUE: The increase in the precipitation which you have noted must be related to unusual changes in the synoptic-scale disturbances. Have you noticed any, or what is different about the large-scale synoptic situations for these recent years?

BARGMAN: Well, I think its true to say that we have absolutely no real models in the tropics for synoptic purposes. Those that we are developing are of such short history that they are almost coincident with the period that we are interested in, and therefore, it is quite impossible for us to say from a synoptic point of view whether, in fact, there have been changes, or not, and we have, in fact, to go to individual climatic parameters to look into this.

GENTRY: Dr. Estoque essentially asks the question I wanted to ask, but let me rephrase it slightly. Have you investigated the rainfall in tropical countries around the world to see if there is any change with longitude with the variation of rainfall with time to see if maybe there is a correlation with certain circulation indices such that one group of longitudes would have excess rainfall, say in 1961 to 1964, and others might have in the other five years of the decade.

BARGMAN: The answer to that, Dr. Gentry, is no. As you know, we are a very small service and, in fact, the work on this problem pressing, because it is domestic, has absorbed virtually 95% of our very small research effort. I don't know whether we can afford to undertake such a problem as you have posed. It is without doubt something that has to be done, which is one reason why prophets from East Africa, like myself, who tour the world, are trying to stimulate interest, so that other people are aware of our problems, that it is possibly of a global scale, and that they themselves will commence looking into it. We can arrive through normal publication at some consensus of opinion, well certainly before another half decade is completed.

KRAUS: There certainly is no correlation between the rainfall in Africa and the monsoon rainfall in India on any long term basis. There is some evidence that these changes which have been observed are associated with changes in the Hadley or in the intensity of the Hadley circulation, but it is a pretty fast speculation. In any case, in what has been said before, Ed Lorentz and I ran a numerical model and you can show quite simply that if there is any change in the forcing function, if you change the external heating and cooling, if there are any external effects, the observable effect in the Hadley circulation would be much stronger than the accumulative effect on baroclinic disturbances in higher latitudes where the noise is very much larger. So that has some bearing on your questions, I think.

FRISBY: Mr. Chairman, one hesitates certainly at this moment to get into any deep discussion of cycles in anything, but certainly over the past years there has been one very interesting bit of work done on a large scale, which did demonstrate some relationship between rainfall amounts and the sunspot cycle. This, though, was outside tropical areas and it seems to me that one of our hopes for the future is that as we get global consideration of some of these problems. We shall be able to demonstrate some relationships between very heavy amounts of precipitation and lake levels in one area with maybe lower lake levels in another. In other words, I think that by degrees, we shall be able to correlate much more easily these very big anomalies of different weather parameters that occur in the tropics, say, balanced against things that are happening outside, and this I am describing on a very large scale. I am not talking about small effects, but I am talking about big things, of the type that you have been discussing, and the thing that I was thinking about, mainly a very large area of the mid-latitudes, the central plains area of the United States. There is no question at all, I think, that there have been very bad droughts related apparently to minima in the sunspot cycle in the early 1930's, in the early 1940's, and in the early 1950's, and when these things are considered over a huge area, maybe there are interrelationships between tropics and mid-latitudes that will be able to be ironed out in the future.

WARM CORE CYCLONES DURING THE SOUTHEAST ASIA SUMMER MONSOON

by

Herbert Riehl
Colorado State University

(presented by M. Orgill)

ABSTRACT

Previous work on the 1961 and 1962 monsoon seasons showed that most important precipitation could be ascribed to one of three types of synoptic weather disturbances:

a) Cyclones that are well established at the ground and in the low troposphere, then decrease upward (assumed warm core);

b) Cyclones well pronounced in the middle troposphere and weak or non-existent at low levels (assumed cold core);

c) Equatorial shearlines without definite center lying across all or most of Southeast Asia, strongest intensity near 700 mb.

Upper air data are far too little for examination of individual cases. Therefore, the approach chosen had to be that of constructing mean fields of the disturbances and then examining variations in individual cases, especially of the weather pattern. This paper gives the results for the warm cyclone, as far as completed. Mean wind fields were constructed to a radius of 7 degrees latitude from the center for 15 cyclones yielding 40 days of data. Convergence, divergence and vertical motions patterns next were constructed for 24 areas of equal size. The level of non-divergence is high, near 350 mb. Next, precipitation was computed in each area using the mean September Bangkok sounding for the boundaries. The resulting pattern showed highest precipitation ahead of the cyclone. After conversion from time in relative coordinates to fixed coordinates on the ground, mass curves of precipitation were computed showing accumulation of 2-3 in. over the three-day period needed by the center to pass over the region at the mean speed of the cyclones.

An individual case is presented relating precipitation from hourly gages, satellite radiation data and streamflow of the Mekong River. This analysis indicates the pattern to be followed for other cases where enough information is available.

The meteorological analysis so far covers the thermodynamics. It is necessary to postulate a surface heat source of reasonable proportions and hot tower ascent in order to permit the outflow to occur at the equivalent potential temperatures required in view of the inflow and outflow computations. Construction of an energy budget and dynamical analysis have been begun but not yet finished.

1. INTRODUCTION

Analysis of weather patterns over southeastern Asia during the summer monsoon seasons of 1961 and 1962 has demonstrated that three types of disturbances produce nearly all precipitation associated with organized rainstorms. Criteria for defining a rainstorm or rain episode have been given in previous semi-annual reports of this project. The three types of disturbances are:

(1) Cyclones readily evident at the surface, extending with undiminished circulation to about 700 mb and then decreasing upward.

(2) Cyclones barely or not at all noticeable at surface, well pronounced in the middle troposphere from 700 to 500 mb and weakening toward the upper troposphere.

(3) Equatorial trough shearlines without definite center, generally oriented more or less east-west. Maximum shear is found in the layer 700 - 500 mb, somewhat variable.

These disturbances, as all others, vanish between 300 and 200 mb, where the primary east to northeast flow continues with very little variation day after day.

A fourth type of disturbance may be mentioned. Clockwise (anticyclonic) flow generally brings suppressed convection, as might be expected; such flow characterizes the "interruptions of the monsoon" which, in southeastern Asia, means westerly flow with the equatorial trough so far north that the rainy zone, if any, is well removed into North Laos, North Viet Nam and China.

It has been one objective of our program to identify the rain bearing and rain suppressing disturbances. The next aim is to give as detailed a description of these systems as is possible; to show their circulation, rain distribution and to analyze the mechanisms by which they are maintained. Ideally, computations should be performed in a representative number of individual cases using 12-hourly chart continuity. In practice, this is quite impossible because upper-wind measuring stations, though far more numerous than in former times, are not sufficiently closely spaced to permit determination of dynamically significant quantities on individual charts. In view of the typically slight temperature gradients, thermodynamic analysis also cannot be undertaken by any direct route. The scatter of the temperatures given by the radiosonde observations at individual constant pressure surfaces is so large, that the variation of temperature with distance from the disturbance centers cannot be reliably determined.

In order to be able to proceed toward the desired objective, it was necessary to make composite charts using all available cases. Up to the present such compositing has been completed for Group I of the disturbances -- cyclones with definite core from the surface to 700 mb and then decreasing upward. We have called these "warm-core" cyclones, although no direct proof exists to this effect.

The following, then, is presented:

(a) Composite horizontal windfields and vertical motion patterns;

(b) Thermodynamic analysis of inflow and outflow, leading to determination of the required surface heat sources;

(c) Precipitation analysis: the precipitation is computed for the mean storm, using the foregoing thermodynamic analysis. Then comparison is made with precipitation determined from hourly precipitation readings, with streamflow of the Mekong River, and with satellite radiation observations. This comparison has been made, so far, only for one storm, and even this one analysis is still incomplete at the deadline for this paper. However, progress up to the time of this writing has been encouraging; and we hope to treat all disturbances with the indicated procedure.

(d) Energy balance and dynamical analysis: work on these strictly meteorological aspects of the mean storm is progressing but not completed.

2. FIELDS OF HORIZONTAL AND VERTICAL MOTION

2.1 Data and methods of computation

There were 15 storms of the "warm-core" type available for analysis during 1961 and 1962. Tracks for the 1961 cases are plotted in Fig. 1. Most storms moved from east or slightly south of east; median speed of propagation was 12-13 kn, as typically found in most tropical areas. The storms had a history of 2-3 days over land where they could be identified; hence the sample of 15 storms yields 40 days for analysis.

Wind observations were next composited at the following levels: 2,000 feet (about 950 mb), 850, 700, 500, 300 and 200 mb. As in previous studies (see Riehl and Byers, 1960) a polar coordinate system was chosen for reference, with origin fixed on the center of the disturbances and with the direction of storm propagation as origin of the polar angle. In this way, the asymmetries with respect to the direction of center motion, often found to be by far the largest part of the asymmetries, are kept in the proper location for the compositing.

After collecting all wind data with the indicated scheme, median tangential and radial components, and resultant winds, were next determined for 24 areas of equal size shown in Fig. 2. The cyclones of southeastern Asia tend to be very large, even though without much wind circulation. This has been noted previously from later passages of these storms over the Bay of Bengal and India-Pakistan; the large size contrasts with the equatorial trough cyclone over Venezuela where, in an analysis similar to this one the disturbance was found to be closed at a radius of 300 n.miles.

2.2 Horizontal wind fields

Figs. 3 - 4 show two of the six mean windfields. At 950 mb an organized circulation is clearly evident, with strongest speeds to the right of the direction of motion, as usually observed in tropical disturbances. At 200 mb, the low-level center has disappeared. Some indication of disturbed flow remains in the right rear quadrant. Otherwise the 200-mb flow direction is mainly along, and slightly across toward left, of the direction of center propagation. Wind speed increases strongly downstream suggesting strong divergence in a current with fairly parallel streamlines. Such divergence would be expected across a disturbance pumping up air from below and containing a large central precipitation region.

2.3 Vertical motion patterns

Subsequent to the drawing of the six composite wind charts, the horizontal velocity divergence in each sub-area of Fig. 2 was computed for each isobaric surface with the well-known formula

$$\frac{1}{A}\oint c_n \,\delta s = div_2 V, \qquad (1)$$

where c_n is the velocity component normal to the boundary, positive outward, δs is an element of the boundary, A is the enclosed area, and $div_2 V$ the horizontal velocity divergence. Next, the equation of mass continuity in constant pressure coordinates was applied, thus:

$$\frac{\partial \omega}{\partial p} = - div_2 V \Big)_p \quad . \qquad (2)$$

Here $\omega = dp/dt$, and p is pressure. Integration was performed stepwise from the ground using proper weights for the spacing of the isobaric surfaces analyzed. At the ground ω was assumed to be zero. The pressure variation along the ground was very weak and the slope of the terrain with respect to the motions analyzed is not systematic; hence, this approximation is considered justified. Further, ω was assumed to vanish at the top of the troposphere near 115 mb. With these assumptions, after integrating around the cyclone at each level, a vertical cross section of ω in the r-lnp plane was obtained, presented in Fig. 5. The result is considered to be quite successful in that a smooth pattern could be drawn virtually without smoothing. Values at the center are far from excessive and not much greater than the mean vertical motions computed by Riehl (1963) for the monsoon season as a whole. However, the level of non-divergence was higher than expected and considerable ascending motion was still computed at 150 mb. These results probably are reasonably accurate, although complete independent verification is not possible. It is known that 50,000-foot tops occur frequently in the region. Further, satellite radiation data (Channel 2) frequently show radiation values of .24 to .20 ly/min or even less, corresponding to effective radiation temperatures of -40° to -50°C. Given an overcast acting as black body radiator, the top of this overcast would be located at 250 - 200 mb considering the temperature-height curves. However, a solid overcast seldom occurs. Thus, it is entirely reasonable to think in terms of a partially cloudy sky with some towers reaching to -60° to -70°C and containing the bulk of the ascending mass; this mass then streams out as cirrus, typically seen on photographs as a fairly thick, shapeless mass.

Direct proof on cloud top heights would depend on RHI radar data monitoring and photography, and on U-2 overflights. Since observations from such sources have not been available to us, we have had to content ourselves with the preceding, somewhat qualitative, reasoning and accept Fig. 5 as probably correct in the first approximation.

Next, a second approximation to the ω values in individual subareas of Fig. 2 was made. It was assumed that, at each radius, the mean value given by Fig. 5 was correct, and that ω could be represented by a single harmonic going around the center at any radius. With these assumptions, sinusoidal curves were fitted to the data of the four rings of Fig. 2 at 950 mb. For the vertical integration, however, equation 2 was used to directly give the change of vertical motion from one level to the next. In this way vertical consistency was preserved, and the levels were not treated independently of one another.

Figs. 6-8 contain three examples of the final vertical motion patterns. Surprisingly, strongest ascent precedes the center, whereas in the case of the Venezuelan cyclone it trailed to its rear. The cause for the difference is not known. But the rain patterns may be influenced by different locations with respect to major mountain ranges, e.g. downflow from the Andes in front of the Venezuelan storm. Since there is no theory specifying where, if anywhere, the precipitation center should lie, we shall be content -- for the present -- to attempt to verify the vertical motion pattern with satellite radiation and hourly precipitation observations.

3. THERMODYNAMIC ANALYSIS

In the following, we shall develop a method of computing precipitation using the vertical motion charts just presented. The method itself has been widely used; it will be necessary, however, to adapt it to the data situation as it exists over southeastern Asia.

We shall assume, in the first approximation, steady state in a coordinate system centered in and moving with the mean cyclone. The divergence of total heat flow through any subarea of Fig. 2 is, in this coordinate system,

$$div(H) = \oint\int (gz + c_p T + Lq)\, c_{nr}\, ds\, \frac{dp}{g} . \qquad (3)$$

Here div (H) is the divergence of heat flow, and div (H) = div $(gz + c_pT + L_q)$; gz is potential energy (g acceleration of gravity, z height); c_pT specific enthalpy (c_p specific heat at constant pressure, T temperature); L_q latent heat (L latent heat of condensation, q specific humidity); and c_{nr} is the outward component of motion across the boundary of the area in moving coordinates. Kinetic energy, strictly, should also be included. This term, however, is always so small in general energy budgets that it can be omitted within the limits of accuracy. A separate kinetic energy balance for the mean storm is being constructed, as stated in the Introduction.

Since we are postulating steady state, div (H) must be zero except when there is a net heat or cold source. The source (Q) is given by

$$Q = Q_s + Q_e - R_a , \qquad (4)$$

where Q_s is sensible heat transfer from ground to atmosphere, Q_e is latent heat transfer, and R_a is the net radiation cooling of the troposphere. With this definition, equation 3 may be subdivided to yield separate heat and moisture balances:

$$div(Lq) = \oint\int Lq\, c_{nr}\, ds\, \frac{dp}{g} = LE - LP , \qquad (5)$$

$$div(h) = \oint\int (gz + c_p T)\, c_{nr}\, ds\, \frac{dp}{g} = Q_s + LP - R_a . \qquad (6)$$

In these equations the release of heat due to precipitation, LP , appears explicitly as does also the evaporation E . Thus, these equations are suitable for computing precipitation, if all other terms can be approximated with sufficient accuracy. A particular problem, which has been encountered previously in attempting tropical heat balances, comes from the difficulty in constructing temperature and moisture fields for the mean cyclone from the radiosonde observations. Indeed, as indicated earlier, this was quite impossible due to the great scatter of the observations relative to the gradients in temperature to be expected. After various attempts, it became clear that simple application of the Bangkok mean September sounding (Figs. 9 - 10) on the outside boundary of each subarea of Fig. 2 would be as accurate as any other procedure. If this is done, almost all variation in precipitation comes from the vertical motion fields and the computed surface heat sources. Since the Bangkok mean sounding is relatively wet to all heights, this is probably a valid approximation. We do not have here the problem of a shallow moist layer topped by a dry inversion, where the moist layer then begins to fill the troposphere under the influence of convergence.

With this assumption, c_{nr} may be replaced by c_n in equations 3, 5, and 6. Since, as stated earlier,

$$\text{div}\,(H) = Q \tag{7}$$

in the steady state system, and div (H) now can be computed using the mass balances in each area of Fig. 2 and the mean sounding of Fig. 10, we can now make an estimate of the total heat source Q . From equations 1 and 2 we know the vertical distribution of net mass flow in or out of each area of Fig. 2, illustrated in Fig. 11 for the left front sector nearest to the center. The level of non-divergence lies near 350 mb. Below, there is a deep layer of inflow, strongest at the ground, as often found in such cases The outflow is centered near 150 mb, a very high level. The moisture distribution with pressure--where the scale for pressure must now be linear for determination of the terms in equation 5-- is shown in the middle of Fig. 11. Multiplication of L_q with c_n will give div (L_q) . It is readily seen, that a strong net inward transport of moisture takes place, since all large moisture values occur at inward directed c_n .

For computation of the total heat source Q the left hand profile of Fig. 11 must be multiplied with $(gz + c_pT + L_q)$, shown in Fig. 10 and alternately represented in terms of equivalent potential temperature (θ_e) on the right side of Fig. 11. Comparing this diagram with the inflow-outflow profile, we see that the outflow takes place at much higher equivalent potential temperatures than nearly all of the inflow. Thus div $(H) \neq 0$, and a net heat source exists which in this case, using equation 3, amounts to 0.49 ly/min . In order for air to flow out at the high θ_e values of 350-360°A, after entering the region with 335-340°A, a mechanism must exist for this air to acquire the additional heat. In as much as no internal source of heat within the atmosphere is known, the only possible conclusion is that the air must acquire the heat through ground-air interaction. In summer the ground is very warm and humid; large areas are water-covered; it is entirely possible for Q_s and Q_e to attain or exceed oceanic values for air-sea heat exchange in comparable regions. If we postulate such surface exchange, expressed in equation 7, we must either transport the heat acquired through diffusion to the middle troposphere, or else let the inflowing air descend to the ground and there acquire the latent and sensible heat while in contact with the ground. While moisture can be diffused upward by small cumuli through the lower troposphere, it is difficult to find a mechanism for sensible heat flow. As suggested by Riehl and Malkus (1958), it is most plausible that the midtropospheric inflowing air descends by means of the thunderstorm downdraft mechanism, acquires latent heat and sensible heat along the ground and then ascends in buoyant updrafts containing "hot towers" that permit vertical penetration to the required altitudes.

Fig. 9 illustrates this scheme. Nearly all outflow takes place above 200 mb. If we consider integrals for the layers 200-150 mb and 150 - 115 mb, respectively, the ascent may be centered along moist-adiabatic ascents with mean equivalent potential temperatures of 352° and 357°A. The downdrafts to the ground surface are also shown in Fig. 9. The air in the inflow will be cooled 2-3°C before reaching saturation, if evaporating precipitation from cumulonimbus clouds falls through it. After this cooling, the air is buoyant downward; assuming enough water is available to keep the descending air saturated to the vicinity of the ground, it will follow the adiabats denoted with downward arrows. If the cloud base remains a few hundred feet above the ground, the descent will reach the ground with about 22°C or 72°F, a very reasonable value.

From equations 6 and 7 we now have

$$Q_s + Q_e = R_a + \text{div}\,(H)\,. \qquad (8)$$

Recent computations and observations on long and short wave radiation indicate that there is a net cooling of about 1°C per day in the layer below 300 mb, but that there is no net radiational heat loss higher up, where radiometer observations have indicated convergence of long wave heat flux. With this information $R_a = 0.11$ ly/min, therefore, $Q_s + Q_e = 0.60$ ly/min. This value is substantially larger than the average net heat absorption by the ground. However, a large heat transfer from ground to air is to be expected in storm situations. Further, the computation refers to moving coordinates, whereas a point on earth remains in the sector analyzed for much less than a day. The sector is one of the most active ones, as may be seen by reference to the vertical motion charts. In most other sectors the upward mass transport is much less, therefore also the heat exchange requirement at the ground and the heat given off by any particular fixed area.

After descent to the surface, the air must, for example, gain 3 cal/gm in order to be able to ascend later along the 352 A moist adiabatic ascent. As pictured in Fig. 9, this can be accomplished by an increase of temperature to 26°C and an increase in specific humidity from 15 to 18 g/kg . In that case, $c_p dT = 1$ cal/gm and $L\,dq = 1.8$ cal/gm, thus Q_s/Q_e is very nearly 1/2, and $Q_s = 1/3$ of the total surface heat source. These values compare closely with those used by Riehl and Malkus (1958) for a similar computation for the equatorial trough zone at large. In the present case Q_s might be much larger, since strong afternoon land heating is available, and since a fair fraction of the land is well above sea level, so that the heat source is elevated. It is also of interest that the water temperature of a river such as the Mekong rises from 25° to 30°C from north to south across the area under study. The circled and shaded area at the bottom of Fig. 9 indicates the region on the thermodynamic diagram from which an ascent may well be started. Thus there is no difficulty at all to make good on the sample ascents of Fig. 9 and to obtain hot tower ascents at θ_e values well above 360°A.

If the ratio Q_s/Q_e = 1/2 is accepted, Q_e = 0.40 ly/min and E = 0.9 cm/day. The moisture balance can now be completed using equation 5. We find that P = 5.5 cm/day and (P - E) = 4.6 cm/day.

4. ANALYSIS OF PRECIPITATION

The computation just described was carried out for all 24 subareas of Fig. 2. Then a precipitation chart could be constructed, presented in Fig. 12 for moving coordinates and in Fig. 13 for stationary coordinates assuming a rate of center motion of 10 knots, which is somewhat slower than the average displacement of 12-13 knots. The principal features of these charts is that the heaviest precipitation precedes the center.

If we consider a fixed rain gauge at the ground which is directly intercepted by the center, a profile of the precipitation rate as a function of time may be constructed from Fig. 12. This profile, expressed in cm/day, is shown in Fig. 14; it is assumed that the rain stops at the center itself. Integrating with respect to time, we obtain the precipitation trace as it would appear on the recording graph of a weighing rain gauge. This trace also has been drawn in Fig. 14. The accumulation over the three days of disturbed weather is 8.5 cm or a little over 3 inches, a very moderate result to be compared with 2 inches computed for the two-day accumulation during passage of the much smaller equatorial trough cyclone over Venezuela (Riehl and Byers, 1960).

When the preceding calculation is repeated at varying distances from the center, total rain accumulation left behind by the storm along a line perpendicular to its path is shown in Fig. 15. If the net accumulation of water on the ground is desired, about 10 per cent must be subtracted for evaporation. This percentage is entirely within the margin of error of all calculations presented; it is therefore not worth while to compute it in detail.

4.1 Verification of computed precipitation

The initial plan was to composite the 24-hour rainfall values recorded at the individual rain gauges with respect to each cyclone. This should give an idea on the accuracy of the precipitation pattern of Fig. 13 and also permit us to discuss variations with respect to the mean storm. Such variations should arise from differences in cyclone intensity, perhaps measured best by the surface pressure difference between outside and inside, and from differences in rate of storm propagation. In addition, the motion of a cyclone with respect to particular mountain ranges will affect the precipitation pattern.

It was expected that the computation as just outlined would lead to total precipitation values well below those of Fig. 13; it is well known that widely spaced rain gauge networks always underestimate precipitation, especially in mountainous territory and in shower-type precipitation. Unfortunately, however, the effort failed entirely. This was partly due to the fact that individual cyclones always passed the land area so that a large portion of the grid of Fig. 2 remained over water, hence was without data. Further, there were difficulties because of heterogeneous station locations. Finally, there was a problem on account of stations reporting zero 24-hour precipitation, when such an observation hardly could have been representative of even small areas around these stations. The only thing we could gain from these statistics was a definite indication that rainfall after center passed tended to be low on the average, in accord with Fig. 13.

4.2 Mekong river data

We have started to explore other sources of information. In particular, detailed streamflow data for the Mekong river have been published (Mekong Hydrologic Yearbook, 1961); this publication also includes hourly rainfall values for a large number of stations in the Mekong drainage, at least for 1961. So far only one case has been analyzed, July 17-19, 1961, because there was an unusual amount of satellite radiation data. The pattern resembled that of Fig. 13 -- most precipitation ahead of, little behind the center. The 3-day integrated precipitation from the hourly gauges for this storm is compared with that from Fig. 15 in Table 1. The result is very satisfactory, both as to the level of precipitation intensity and as to

Table 1

Comparison of 3-day precipitation from hourly rain gauges July 17-19, 1961, with precipitation computed for mean storm (unit cm/3 days).

Location (° lat. from center)	Mean storm	July 17-19
3° N	2.0	4.0
2° N	6.5	2.6
1° N	8.0	7.4
1° S	8.0	5.5
2° S	4.5	4.7
3° S	1.0	1.8

pattern. There is only one large disagreement, in the region centered 2^{o} latitude north of the center. In this area low level winds were easterly, so that the Mekong basin experienced general downdrafts as a lee effect from the coastal mountains in the east.

In spite of this encouraging first result, analysis of precipitation in tropical regions using routine networks will never be very satisfactory. It is very appealing to try to utilize streamflow data, as we also have done for the Venezuelan cyclone. The discharge data for the Mekong provide an opportunity. Fig. 16 shows the location of stream gauging stations used, as well as the channel 2 infrared radiation measured by TIROS III. We are interested here in the drainage areas between gauging stations, since the difference in runoff between stations may give information on integrated storm precipitation over these drainages. The size of the basins, in 10^3 km^2, is as follows:

Luang Prabang to Thakek	105
Thakek to Pakse	142
Pakse to Kratie	101
Kratie to Phnom Penh	17

The drainages are of comparable and, for our purposes, useful size except the southernmost one, which we may evidently discard.

Glancing at a record of streamflow at each of these stations over the 1961 monsoon season (Fig. 17), the streamflow responds well to storms 1, 3, 4 and 6, which passed directly over the area. Storm 2 was far to the south (see Fig. 1) and storm 5 was dying out over the Mekong basin. Of course, Fig. 17 shows other features; but then we have only analyzed one of our three classes of disturbances to date. It is of particular interest that the amplitude of the rises and falls in the streamflow increases strongly from north to south. This is even more marked for 1962 (not reproduced) when Luang Prabang showed only one gradual rise and recession, reflecting the Himalayan snow melt. In view of the time needed by the water to travel downstream, the rises and falls occurring at the southern stations during periods of a few days must be interpreted as produced by locally occurring rains. Clearly, then, the major water delivery from storms took place mainly south of Thakek.

Fig. 18 shows the streamflow recorded at each of the stations during part of July 1961, centered on the storm of July 17-19. The difference in streamflow between succeeding stations also has been plotted. It is seen that streamflow responded to the storm at all stations, least at Thakek where, as already mentioned, a lee effect from the eastern mountains was experienced. The amplitude of the rise in discharge subsequent to the storm increased from north to south, most of all at Kratie where the discharge due to the central portions of the storm was experienced. This is in agreement with Table 1.

The transformation of a streamflow record into precipitation over the drainage above the recording gauge is not an easy matter. We have started to apply hydrologic techniques with advise from hydrologic engineers at Colorado State University. The result of these studies should be available for our technical report next November. For the present, it has been possible merely to consider the elevation in the difference of streamflow between Thakek and Pakse, and between Pakse and Kratie, over previous levels during July 19 to 24. The increase in runoff in the northern basin was 1 cm, in the southern basin 5.5 cm. These values are entirely in the range of those in Table 1, even though complete recession curves have not yet been determined, and even though only a fraction of the precipitation enters the stream. The result from this preliminary attempt is considered sufficiently encouraging to warrant pursuing utilization of the Mekong streamflow data for our purposes in considerable detail.

4.3 TIROS III radiation data

The analyses of Figs. 16, 19 and 20 show the channel 2 window radiation (ly/min) as determined with the electronic computer program developed at Colorado State University for this purpose. Computations were performed at the Western Data Processing Center, located on the campus of the University of California at Los Angeles.

Perhaps the principal feature revealed by these charts is that an area of low radiation (.24 ly/min) accompanied the cyclone across southeastern Asia. Further, broad banded structures of high and low radiation are in evidence, for instance a broad, counterclockwise curved band passing from Burma to the South China Sea on July 17. On this day the indications of the lee-in-the-mountains effect north of Thakek are also most pronounced, with high radiation (0.36 ly/min) in the Mekong Basin and low radiation (0.20 ly/min) on the eastern slope of the mountains. On the 18th, the low radiation spread westward; however, the strong gradient from high to low values remained stationary on the eastern mountain slope, until the cyclone passed off.

For a quantitative picture of how radiation changed with time, the analyses of Figs. 16, 19 and 20 were integrated over the several drainage areas of the Mekong River, with results as given in Table 2. The time of the observations was near 1900 GMT.

Table 2

TIROS III Window Radiation Values (ly/min)

	July 17	18	19
Luang Prabang to Thakek	.36	.30	.34
Thakek to Pakse	.32	.30	.32
Pakse to Kratie	.30	.25	.33
Kratie to Phnom Penh	.32	.25	.34

The results of Table 2 coincide well with those of Table 1 and Fig. 18. Evidently, as far as data permit, analysis of storm precipitation will be pursued in terms of these three sources of information.

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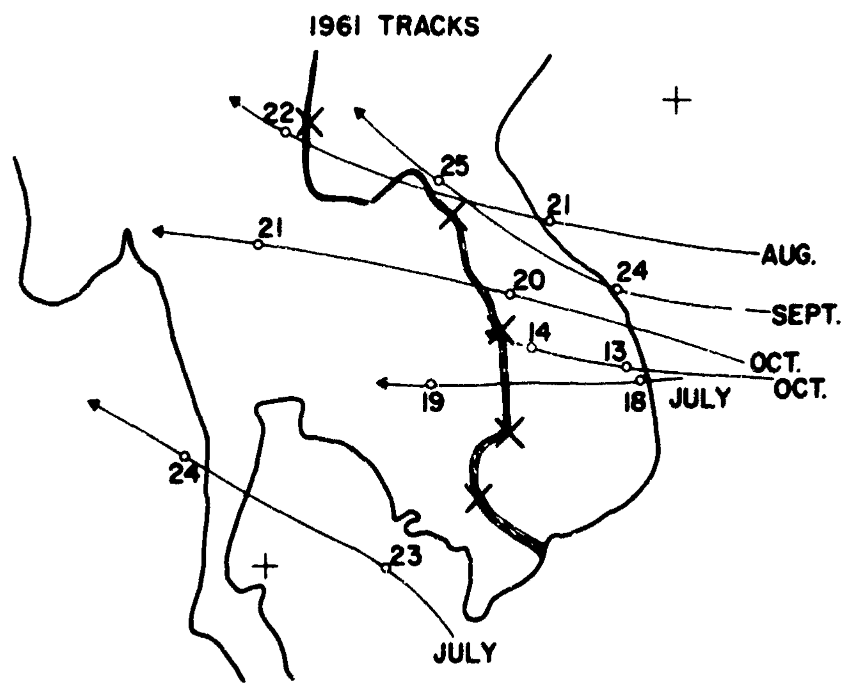

Fig. 1 Tracks of six "warm core" cyclones during 1961

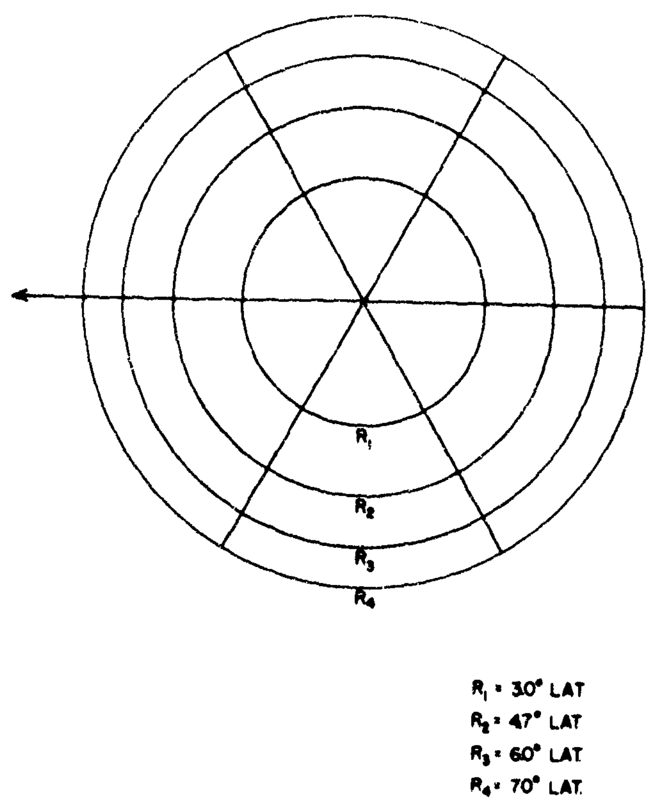

Fig. 2 Division of whole area of cyclone into 24 subareas for averaging. Heavy arrow denotes direction of propagation

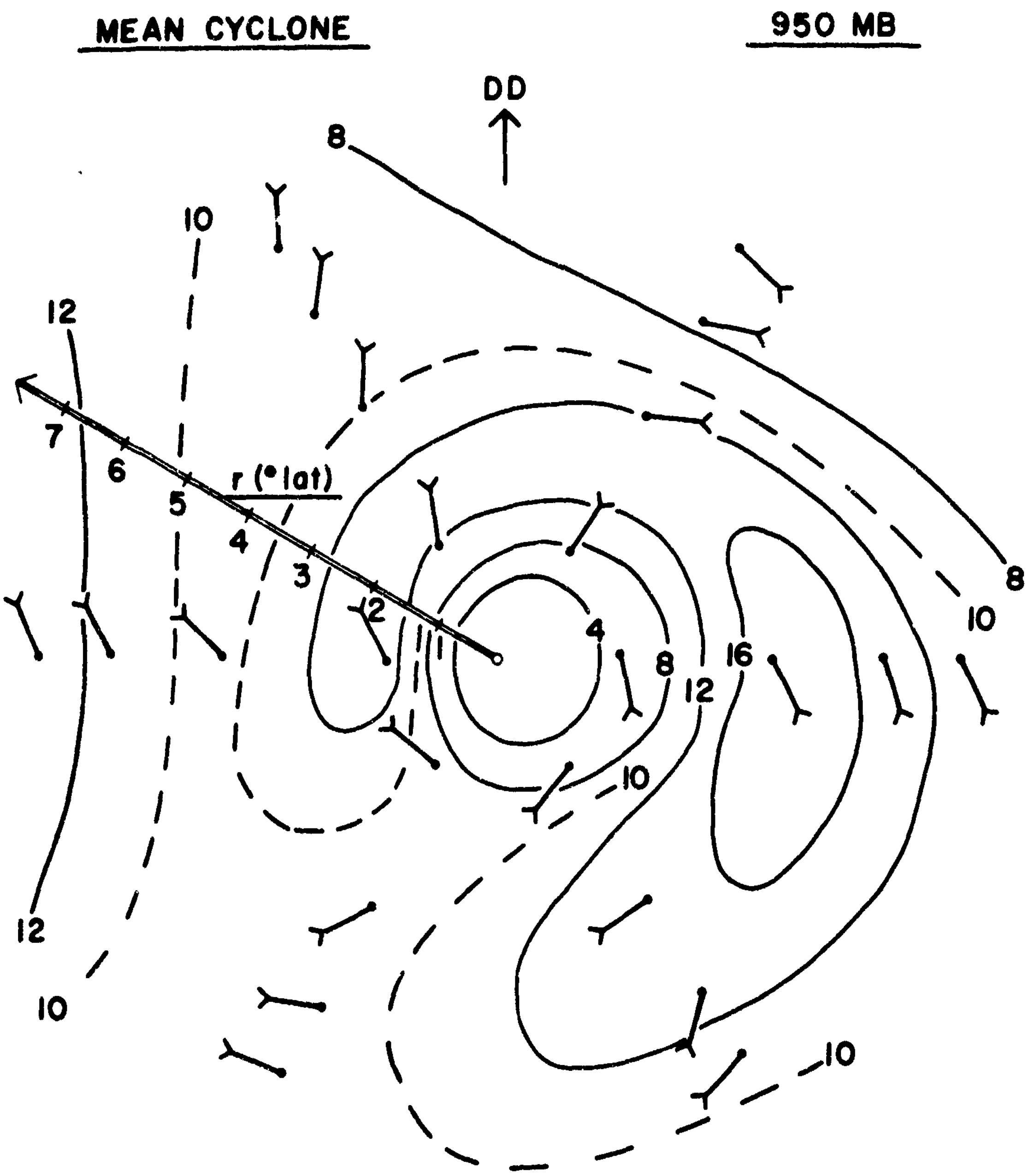

Fig. 3 Composite 950-mb windfield. Speed in knots

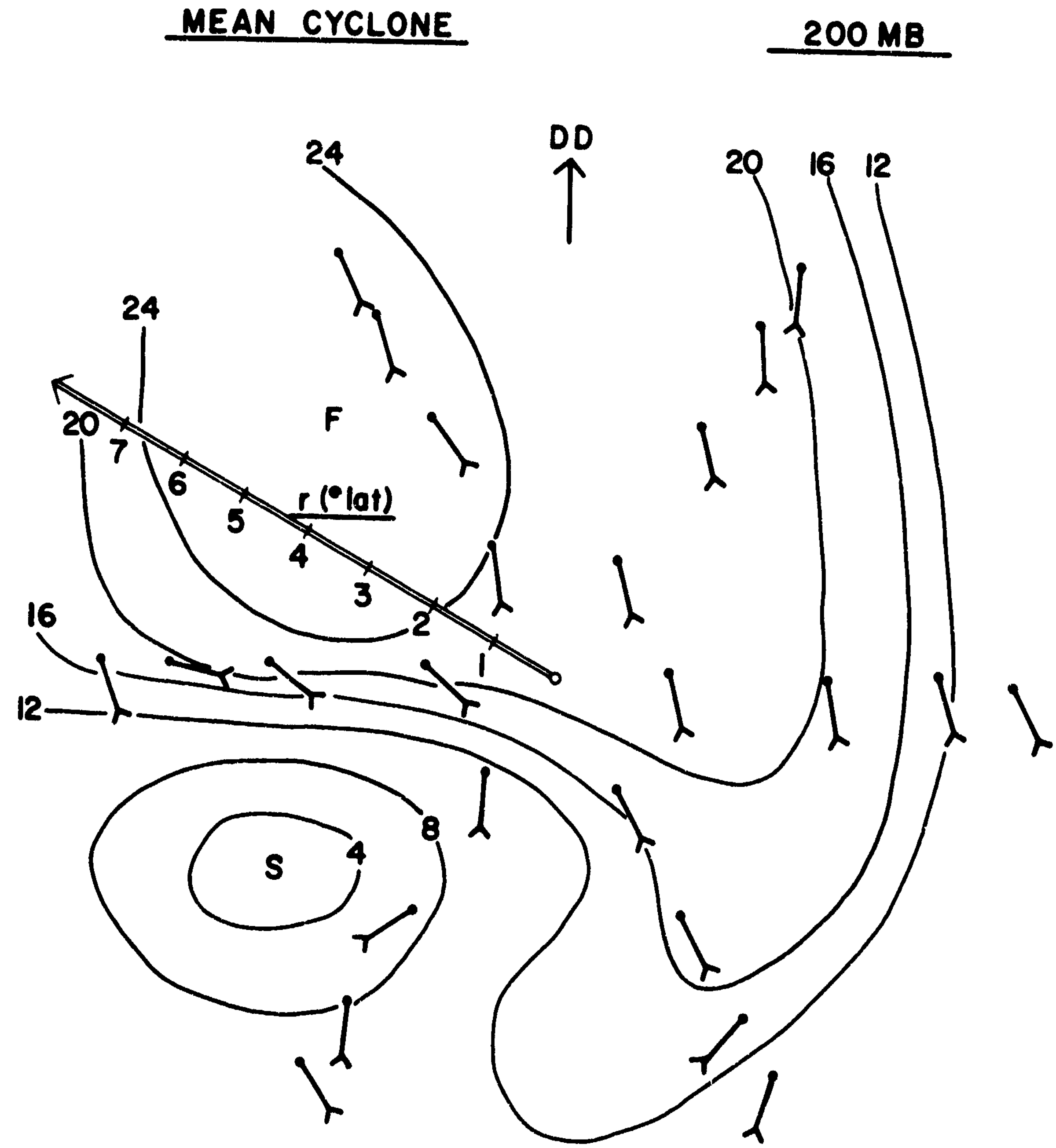

Fig. 4 Composite 200-mb windfield

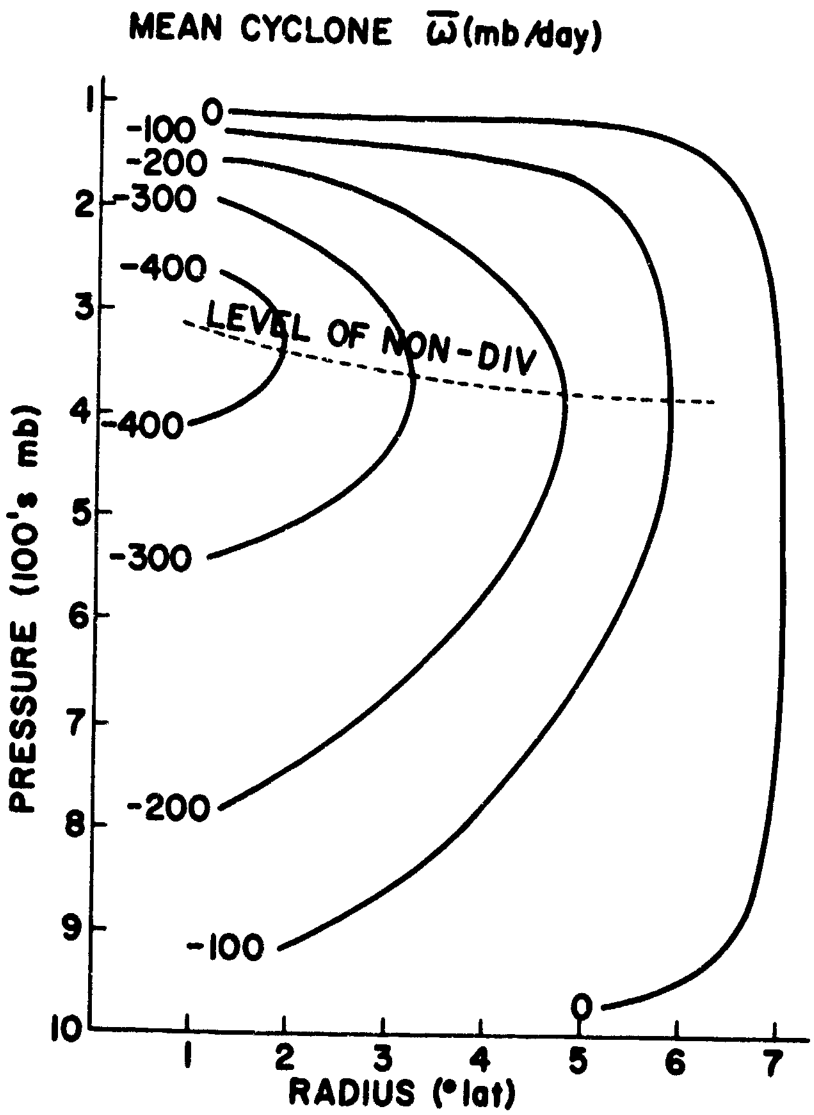

Fig. 5 Radial profile of vertical motion averaged around mean cyclone. Vertical motion in mb/day, negative sign denotes upward motion.

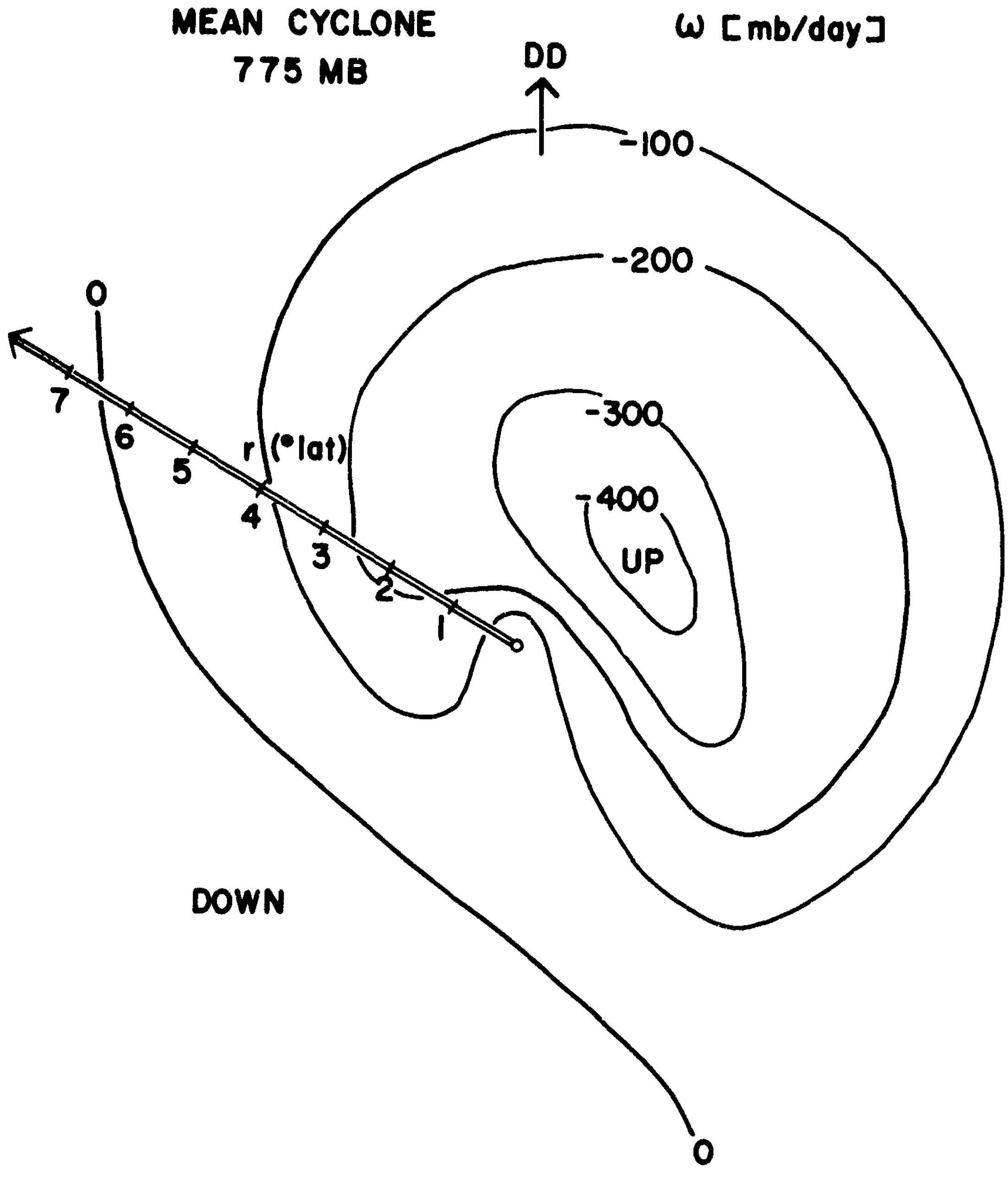

Fig. 6 Vertical motion (mb/day) for mean cyclone at 775 mb.

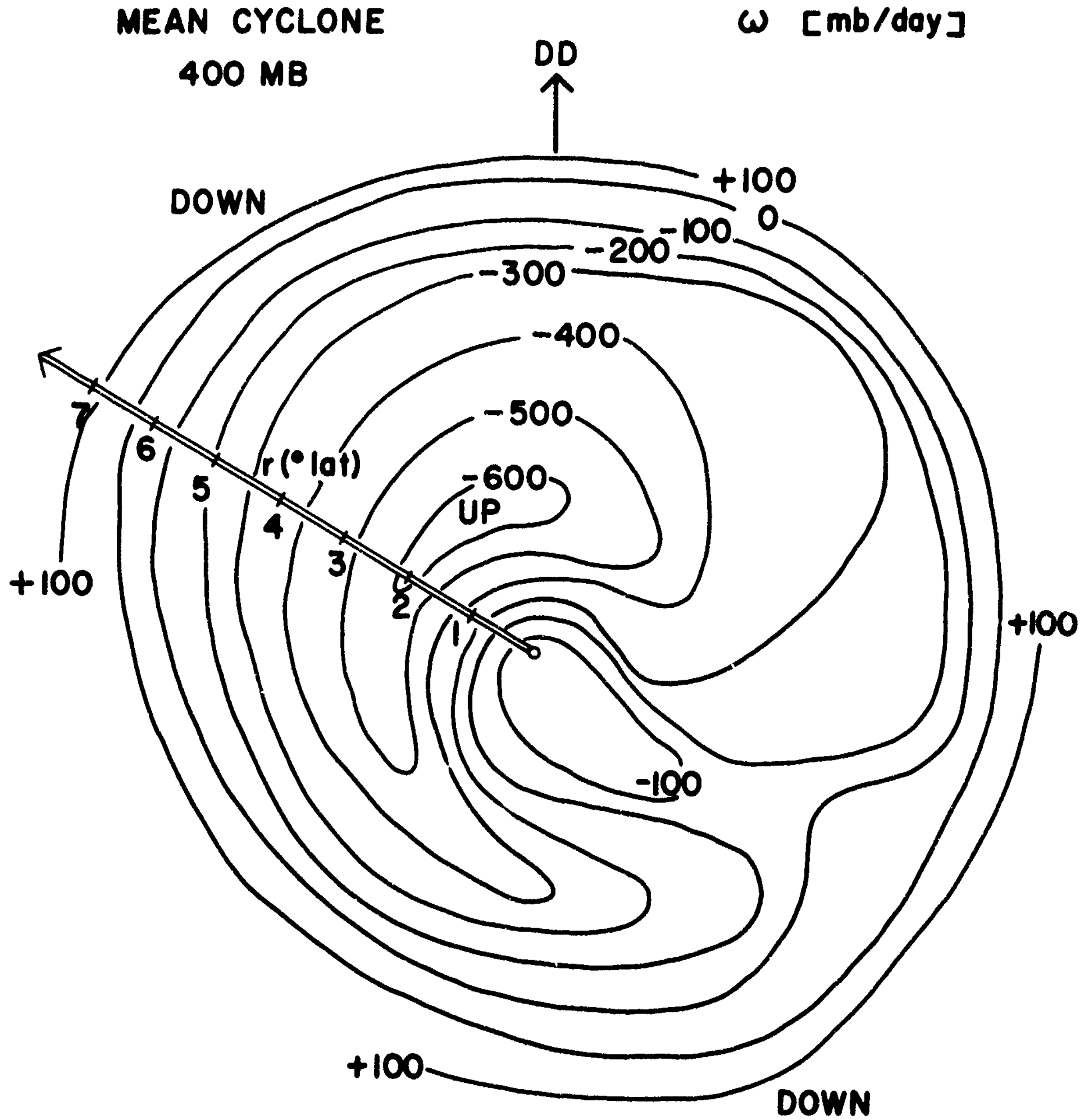

Fig. 7 Vertical motion (mb/day) for mean cyclone at 400 mb.

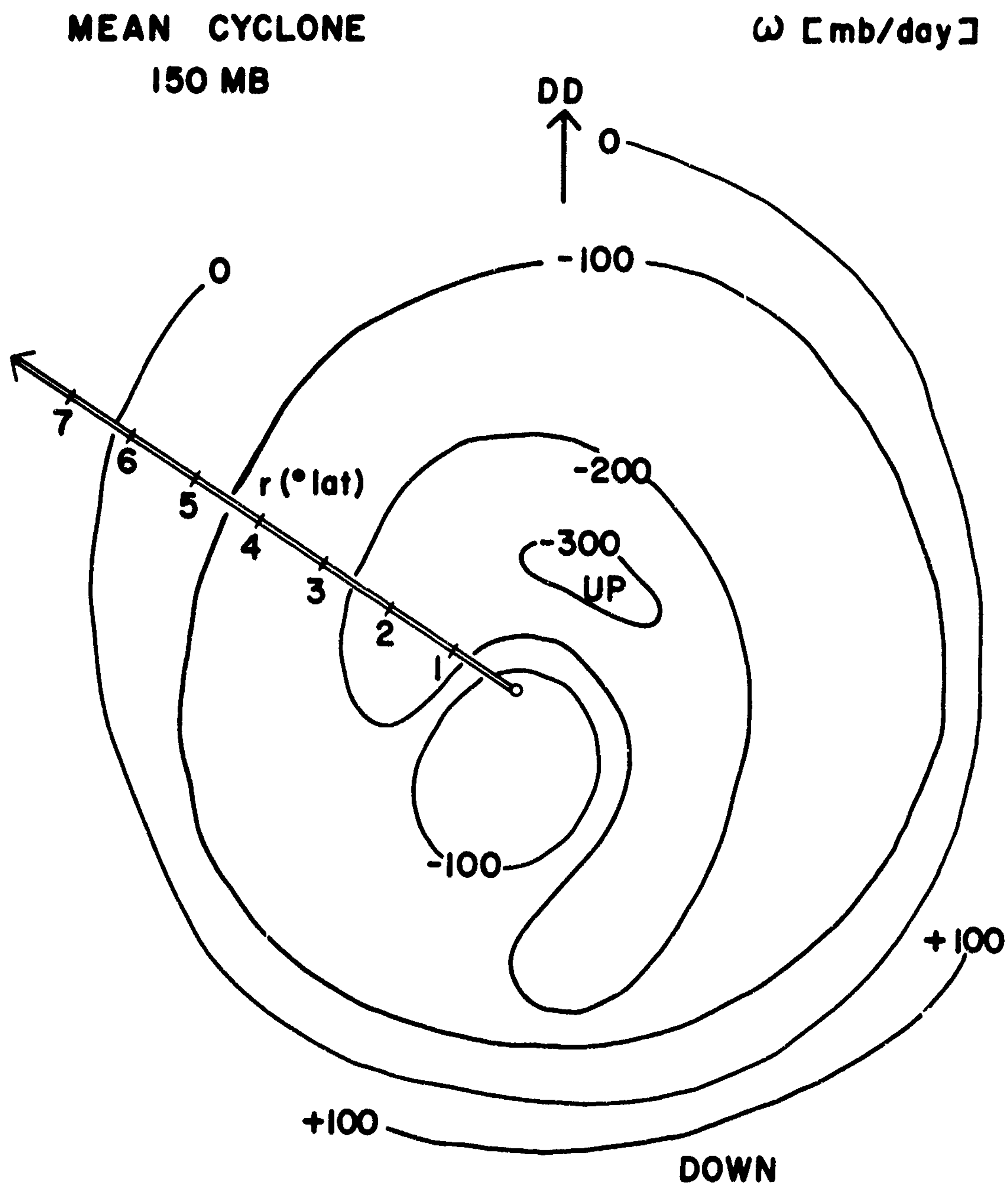

Fig. 8 Vertical motion (mb/day) for mean cyclone at 150 mb.

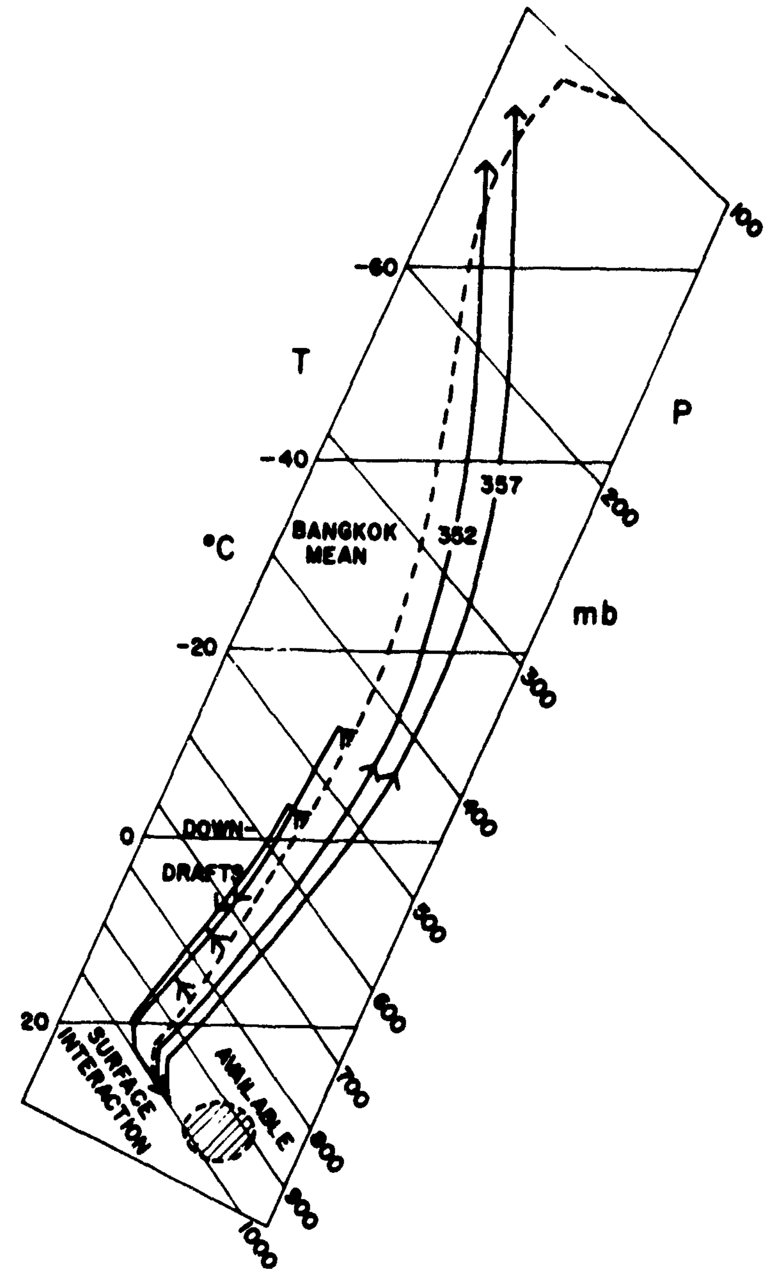

Fig. 9 Tephigram showing Bangkok mean sounding (dashed); ascent paths of air required for high-tropospheric outflow in layer 200-150 mb and 150-115 mb; and paths of downdrafts assuming mid-tropospheric inflow is accelerated to surface in thunderstorm downdrafts.

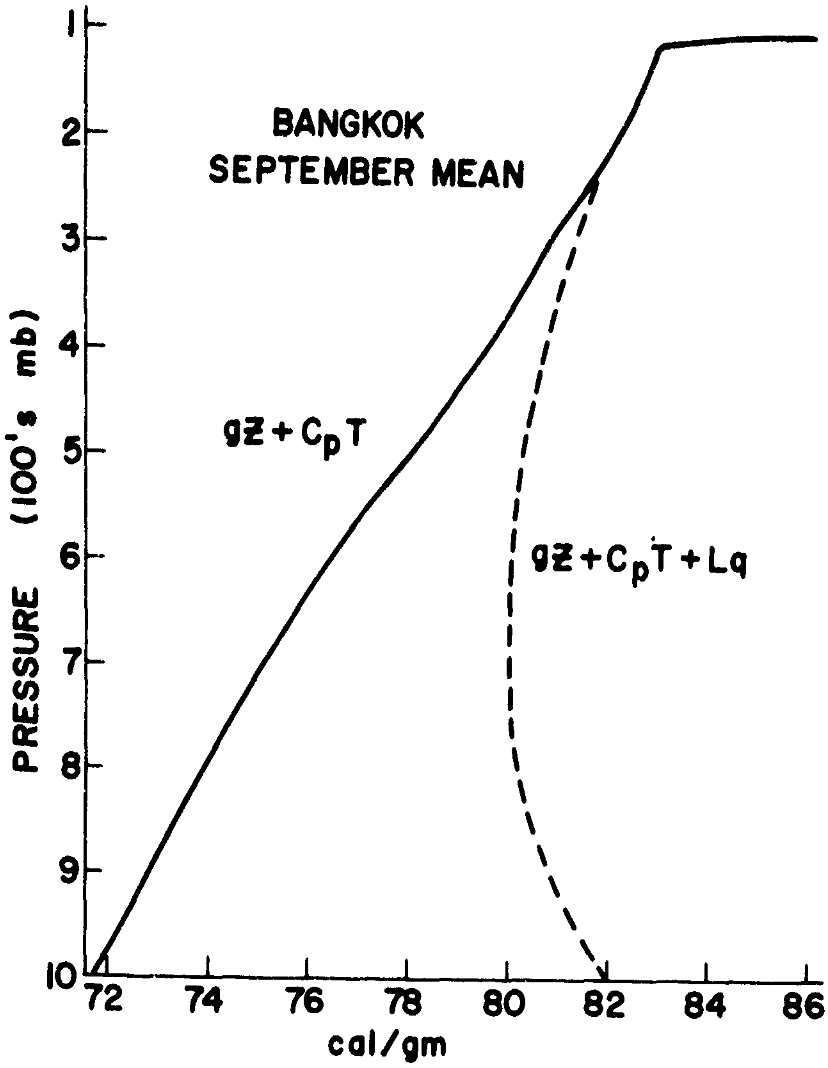

Fig. 10 Distribution of sensible and total heat with pressure for Bangkok mean sounding

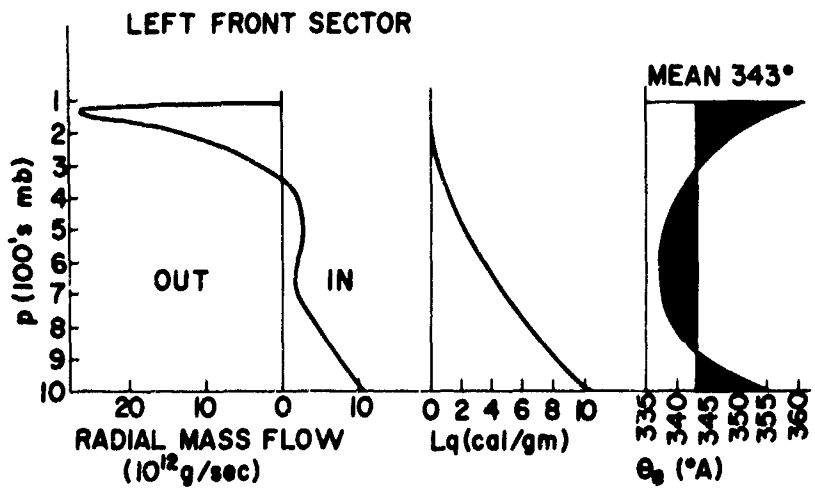

Fig. 11 Left front sector of cyclone: left, inflow-outflow profile; center, profile of latent heat energy of Bangkok mean sounding; right, equivalent potential temperature distribution with pressure for Bangkok mean sounding.

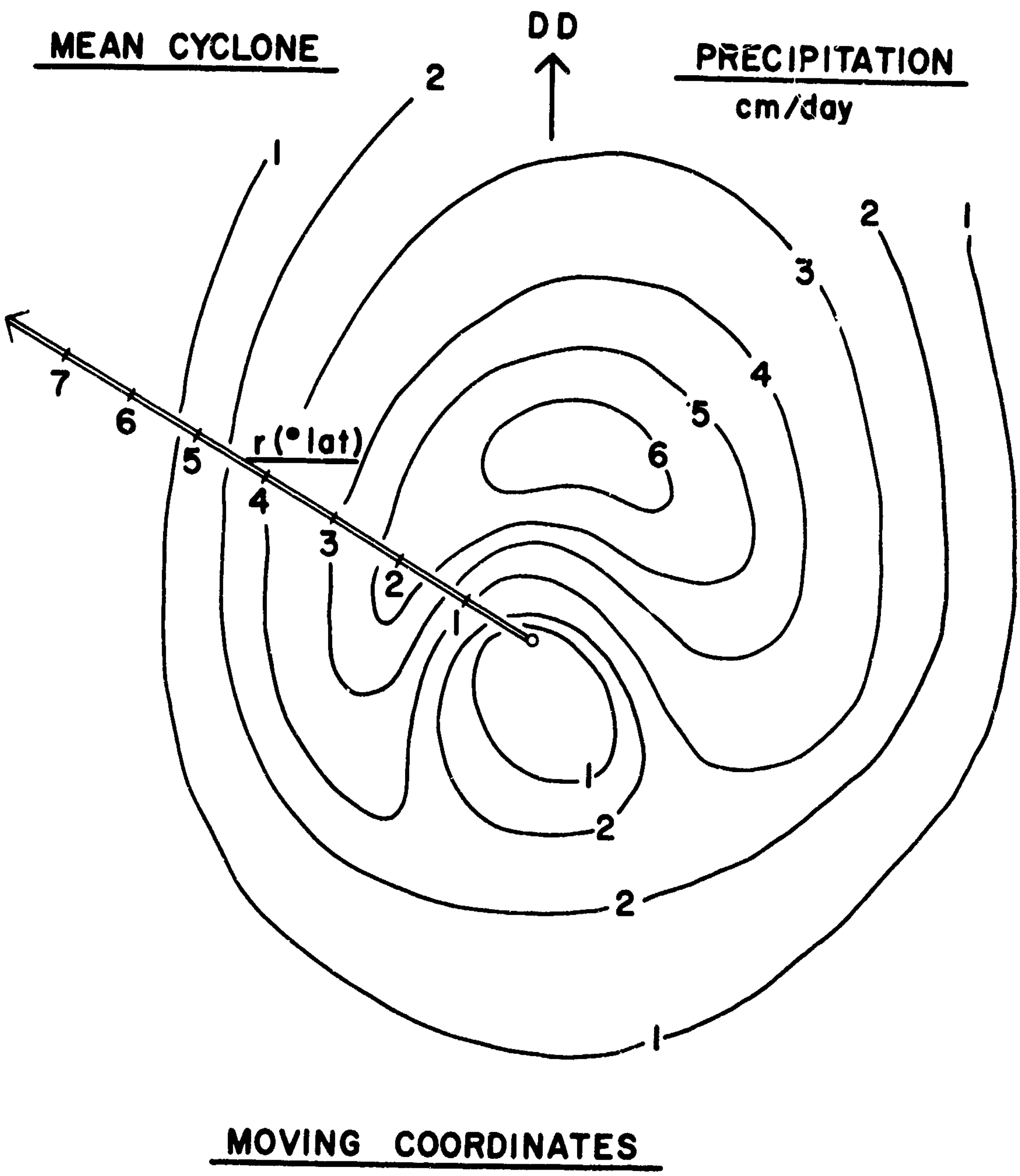

Fig. 12 Precipitation (cm/day) for mean cyclone in coordinates attached to cyclone center.

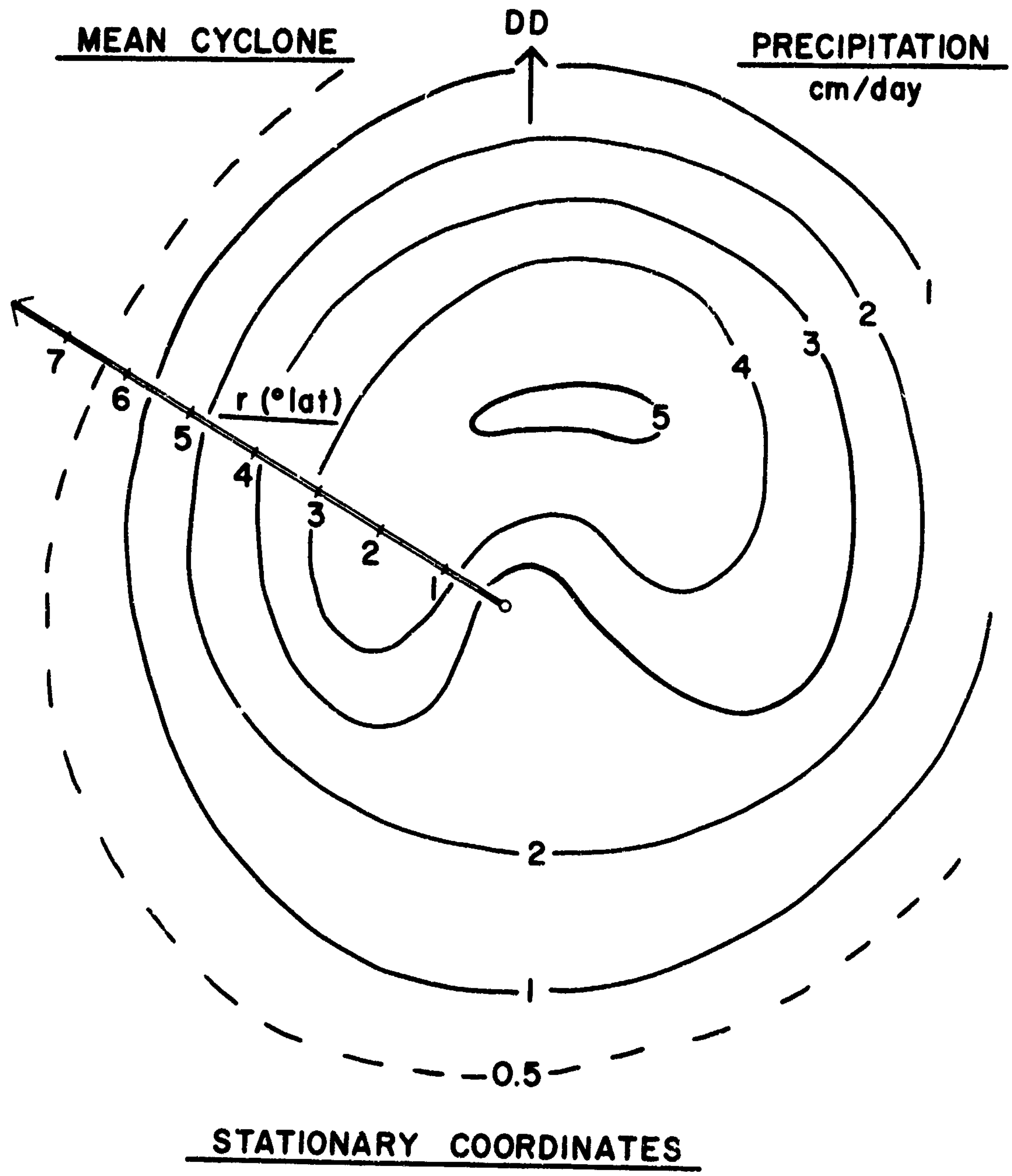

Fig. 13 Precipitation (cm/day) for mean cyclone in stationary coordinates.

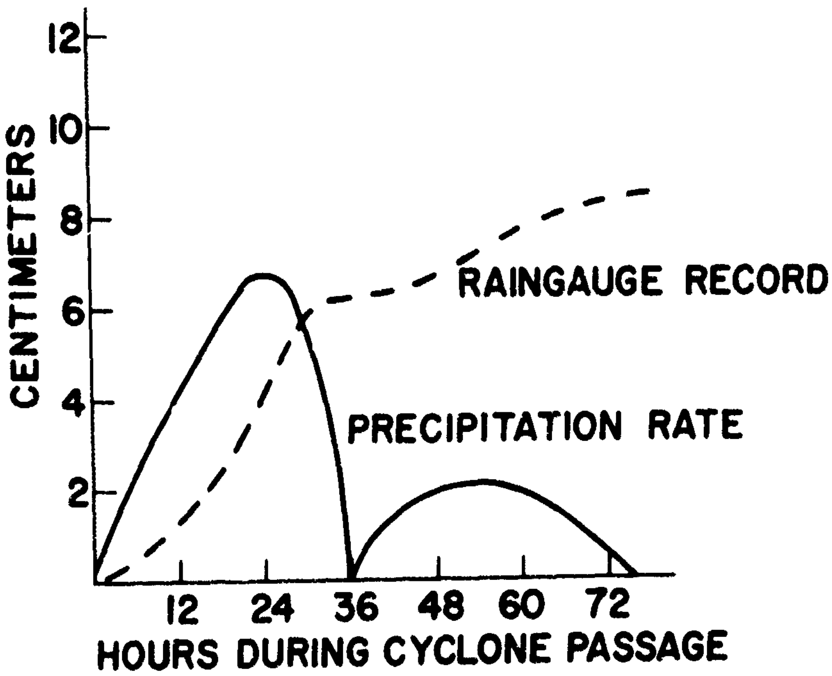

Fig. 14 Profile of precipitation rate (cm/day) and of precipitation accumulation (weighing rain gauge trace) for gauge passed directly by center.

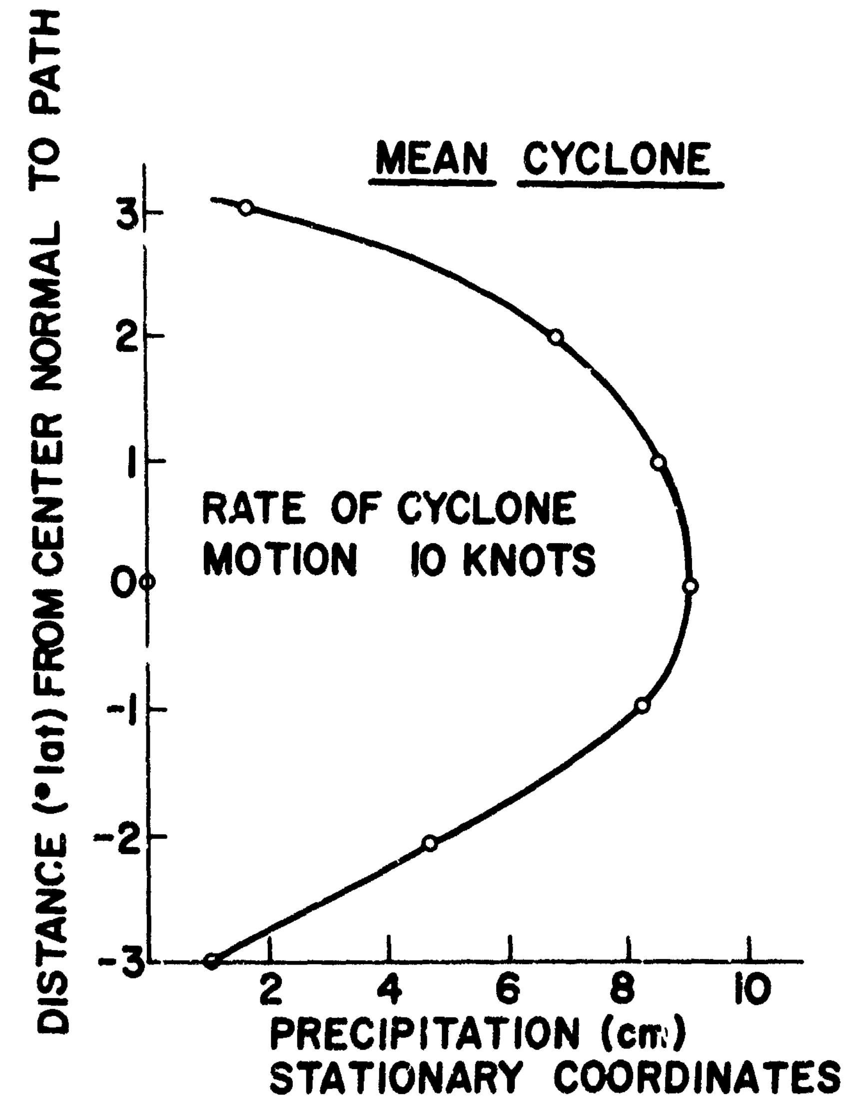

Fig. 15 Accumulation of precipitation (cm) by cyclone along profile perpendicular to direction of cyclone motion.

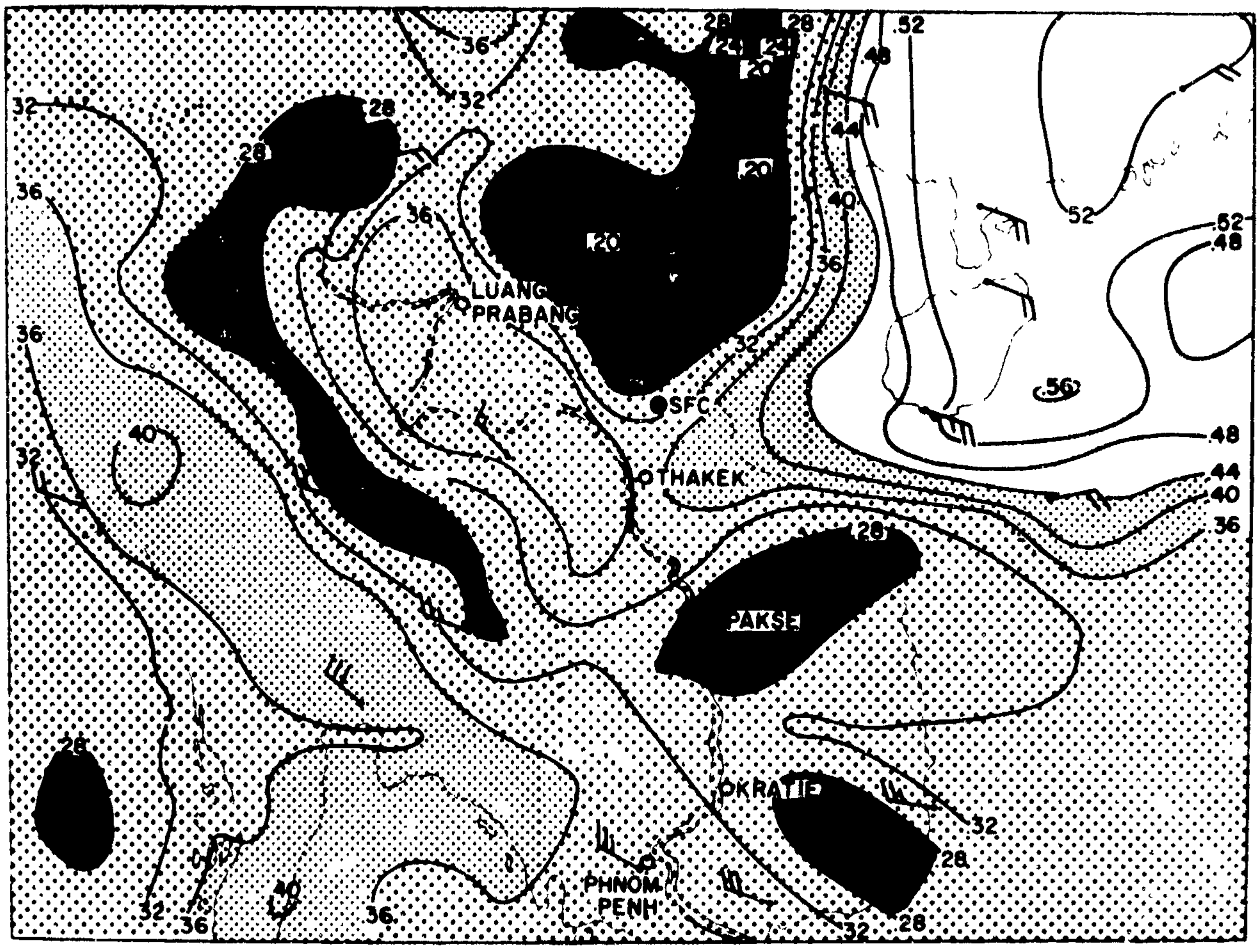

Fig. 16 The Mekong River basin and gauging stations used. The 700-mb winds outline cyclonic circulation. TIROS III, channel 2 radiometer observations at 1900 GMT on 17 July 1961 (ly/min).

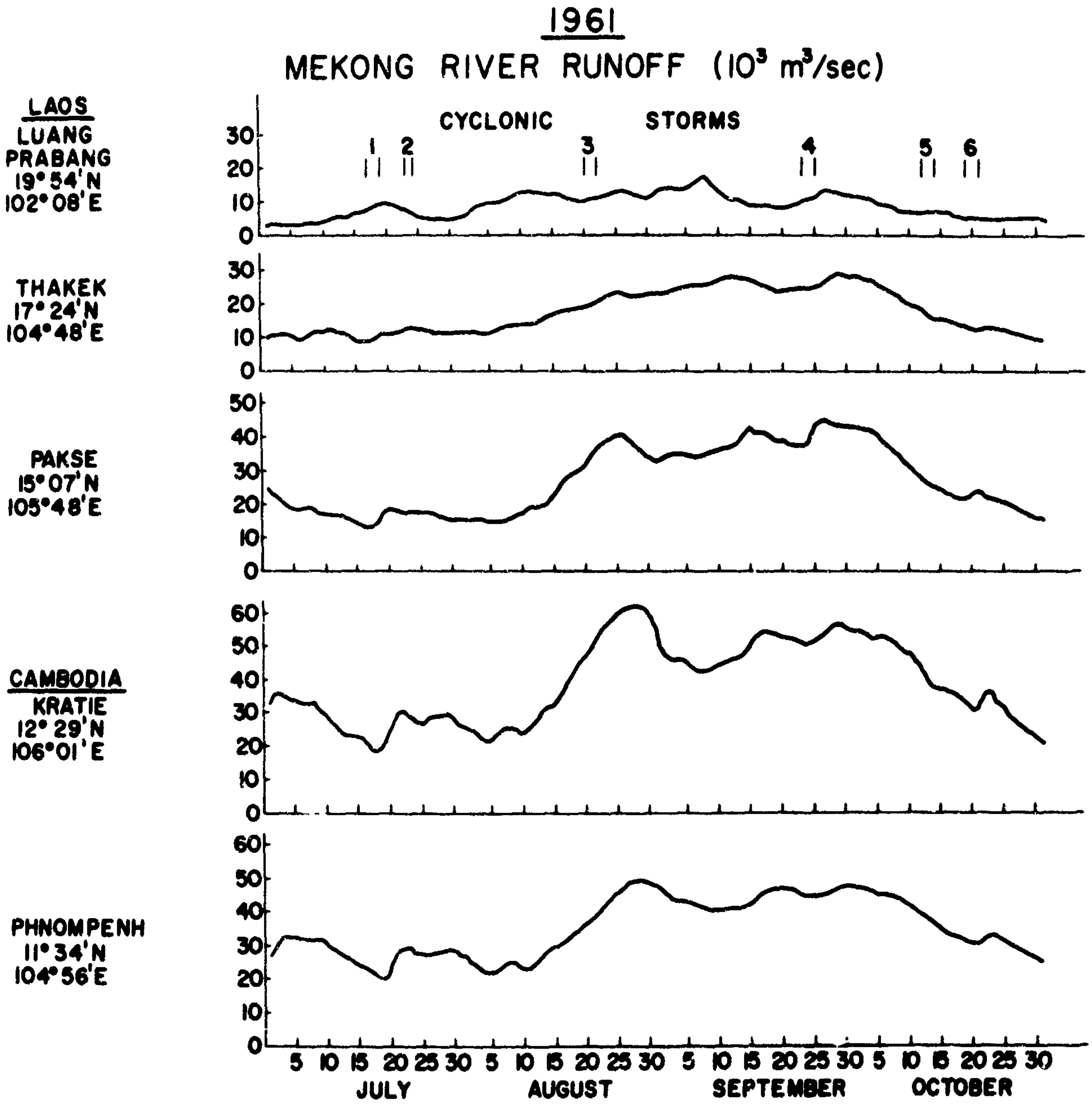

Fig. 17 Daily discharge of Mekong River (10^3 m^3 sec^{-1}) at the stations shown in Fig. 16 for 1961 monsoon season.

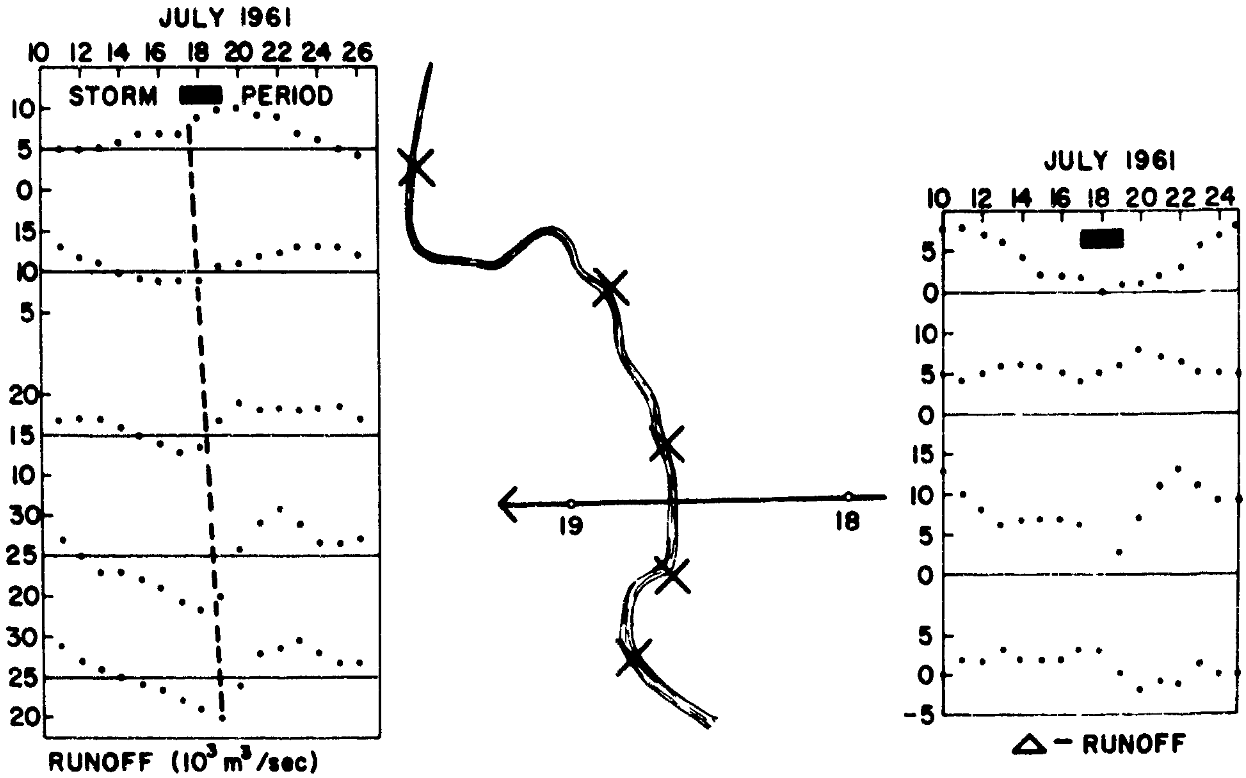

Fig. 18 The Mekong River Basin and the five gauging stations, repeated. Left, daily discharge at these stations (10^3 m^3 sec^{-1}); right, difference in discharge between successive gauging stations.

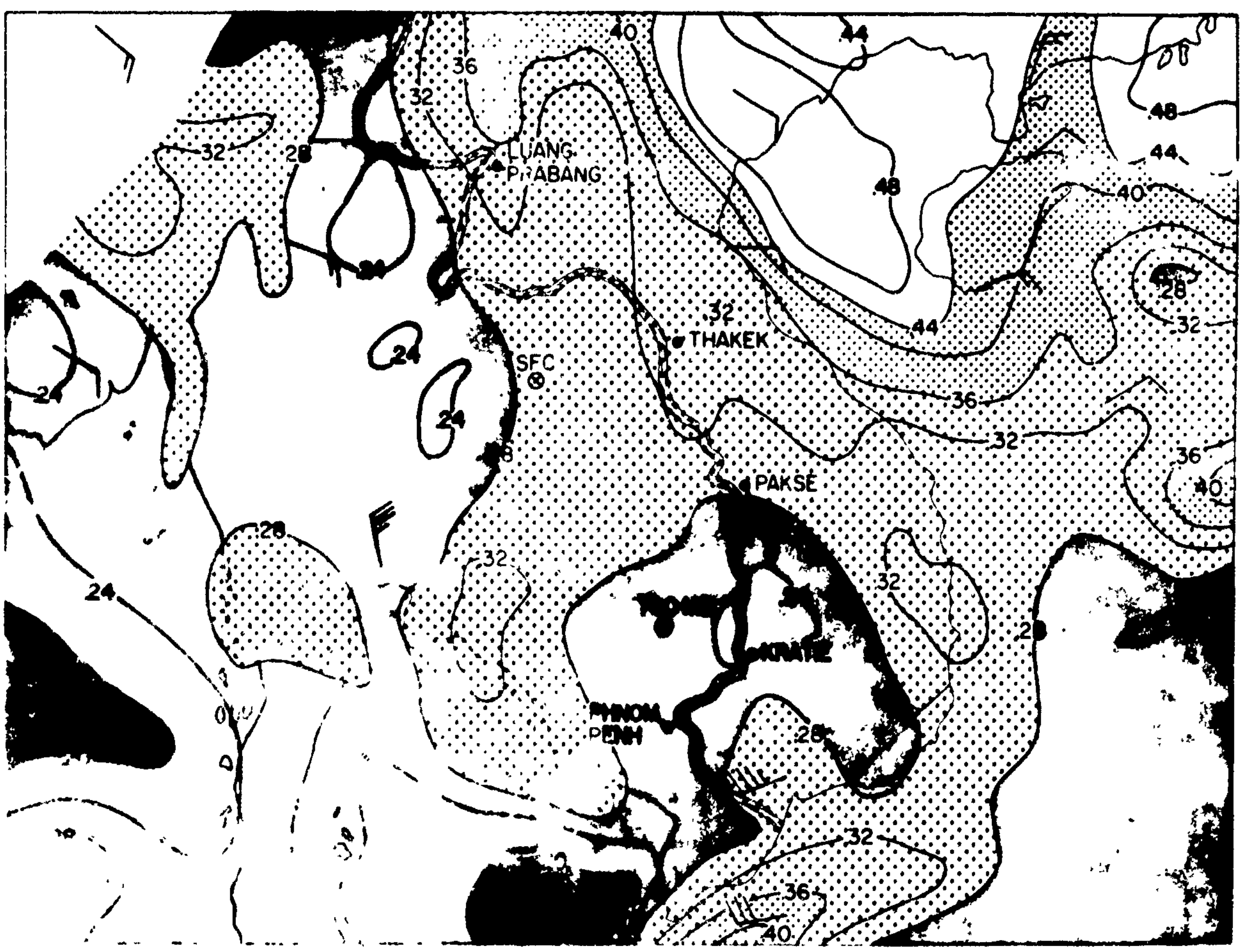

Fig. 19 TIROS III, channel 2 radiometer observations for 18 July 1961. Notation as in Fig. 16.

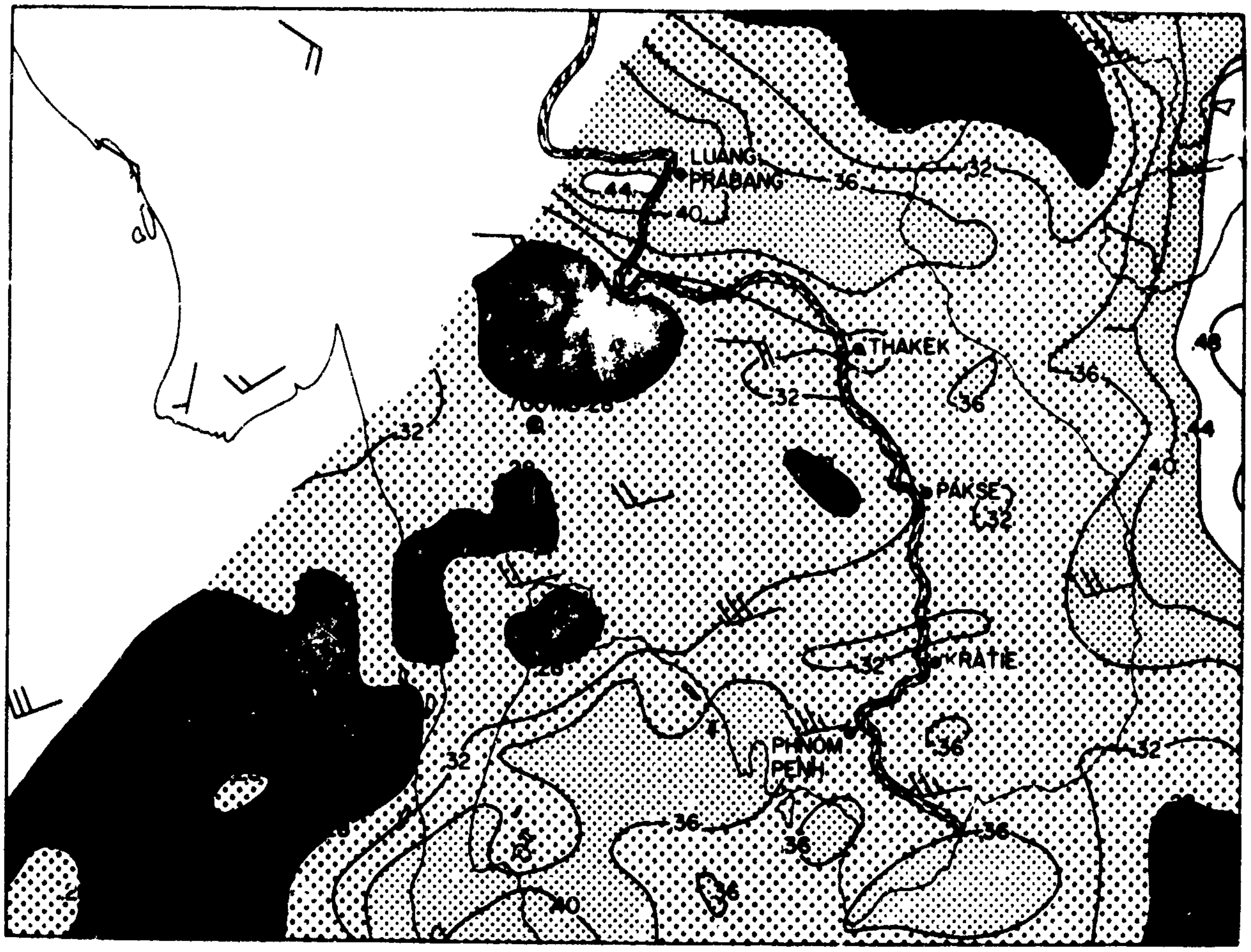

Fig. 20 TIROS III radiometer observations for 19 July 1961.
Notation as in Fig. 16.

DISCUSSION ON RIEHL'S PAPER

KOTESWARAM: I was very much interested, Mr. Orgill, in the fine paper of Dr. Riehl. In fact, I should compliment your group on the excellent results from the very meager data because I am quite aware of the lack of adequate data in that particular area. Now, my interest has been in the kind of similarity between what you have in that part of southeast Asia and what we call monsoon depressions in India. I guess that the type of disturbances which you have investigated are about the same type as what we call the monsoon depressions in India. In fact, you yourself have mentioned that some of them go into the Bay of Bengal in some cases and get regimented and intensified into monsoon depressions. I have been looking forward particularly for any evidence of these warm core systems. We have a lot of data in India, however, we could not see that there was any warm core, as such, that is, warm areas surrounded all around by cold areas. We, therefore, came to the conclusion that they are not warm core disturbances as such, but could be called thermal waves, or thermal troughs. This particular idea also helps in explaining the fact, which you have noticed here, that you have rainfall ahead of the storm, particularly in the southwest quadrant. That's a well-known fact with regard to the Indian monsoon depressions. You have the maximum rainfall in the southwest quadrant and the rainfall amount is quite high, maybe 10 inches per day, but apart from the actual figure, the way in which it has come out in your computations does show that there is great relativity in the way in which it has been worked out. We are not quite sure whether these depressions really have the warm core because they are completely overridden, as you have pointed out, by strong easterlies which increase warm cores. The disturbances extend up to 700 millibars and when they are intense, they extend up to 300 millibars. Above that they are just overridden by these easterlies, and I personally in my papers have been inclined to believe that it is not a fully warm core disturbance, but it is a sort of thermal trough which causes it.

GOLDMAN: On your slide that showed the omegabar, I noticed you had a zero for omegabar and then all the values were negative, the zero extending out to 7 degrees radius. Was there any computed positive divergence out beyond 7 degrees so that you could get a positive omegabar?

ORGILL: These storms that occur in this area are quite large, that is the circulations do extend out to 7 degrees latitude, and the only positive values did occur about after that latitude, 7 degrees. All the rest were negative. On occasions the disturbances are not that large, but in compositing them, this is the way it came out, the negative values did extend out to about 7 degrees. In reply to Dr. Koteswaram about the warm core definition: we have just assumed this; we have no way to verify it. We just postulated this on the basis of the circulation and, of course, in order to verify it, we would have to have better data, perhaps flights through these storms like they do with the hurricanes and typhoons.

New Inter-Oceanic Canal Feasibility Study--Meteorological Program

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ABSTRACT

Plans to investigate the feasibility of constructing a sea-level inter-oceanic canal, including the possibility of using nuclear excavation techniques, call for the establishment of a two-year data collection program in eastern Panama and in northern Colombia. An extensive meteorological program including radar wind stations, weather surveillance radar, synoptic surface observations, wind towers, rain gages and tetroon-tracking is planned.

1. INTRODUCTION

The Congress has recently authorized an investigation to determine the feasibility of constructing an inter-oceanic sea-level canal in Central America. The studies are to evaluate both conventional and nuclear excavation techniques. Enlargement of the present Panama Canal and the selection of other routes through Central America are being considered. The bill authorizes the President to appoint a five-member commission to make a full investigation and reports its findings and conclusions to the President and Congress. Pending the appointment of this commission, preliminary plans for the study have been made. This phase includes a two-year data collection program at proposed routes in the Darien region of Panama and in Colombia near the Colombia-Panama border. The Atomic Energy Commission is expected to be a major participant in the site surveys, having primary responsibility for developing a nuclear excavation plan and for evaluating the hazards involved in nuclear excavations. Since the Weather Bureau has provided meteorological and fallout prediction support to the AEC for nuclear test operations over the past 10 years, the AEC has again turned to us for support in connection with this problem.

The concept of digging a canal with nuclear explosives is to detonate a series of buried nuclear devices along the canal alignment forming a row of inter-connected craters which would serve as the ship channel. The tremendous energy produced in nuclear explosions would be used both to fracture the material and eject it from the channel. The feasibility of nuclear excavation depends on the ability to blast craters of the required size at a cost competitive with conventional techniques. A part of the comparative cost analysis of nuclear and conventional excavation techniques is the relative cost of the necessary safety programs that must be an inherent part of such an engineering operation.

2. METEOROLOGICAL PROGRAM

The feasibility of nuclear excavation is related to the frequency of occurrence of predictable weather situations which can be depended upon to confine biologically significant levels of radiation within controlled areas to assure the safety of participants and local populations. The distribution of the radioactive fallout depends upon the direction and speed of the winds from the ground to the altitude of the top of the radioactive cloud and on the precipitation occurring in the area transversed by the cloud. The propagation and intensity of the air blast waves from the underground explosions are also dependent on the wind and temperature structure of the atmosphere at the time of detonation. The feasibility study consists of two years of site surveys followed by six months of data analysis and report preparation.

For the site survey, four major meteorological stations will be established in the jungles of Panama and Colombia, two along each of the proposed routes (Fig. 1). These stations will make routine four-a-day synoptic surface observations and radar wind runs to 60,000 feet. The two stations at the Pacific ends of the routes will also have weather surveillance radar and will make frequent determination of the precipitation patterns and cloud heights. The details of the weather surveillance program have not been worked out and, in fact, we would welcome suggestions from this audience.

We intend to use M-33 radars, the X-band will be used for the wind finding and hopefully to determine cloud tops. The S-band will be used for weather surveillance. We would like very much to incorporate precipitation intensity study in our weather surveillance program. An attempt will be made to locate recording rain gages in the vicinity to serve as calibration points for our radar. A tetroon-tracking program is also contemplated to study the flow patterns across the Isthmus at several levels. It is intended to launch balloons on the upwind side of the Isthmus and follow them completely across the Isthmus passing the tetroon from one radar to another. To enable us to make a detailed analysis of the low level wind field and to gain some insight into sea-breeze and other local effects, several 100-foot wind towers will be established along each of the routes. These towers may be moved to other locations during the course of the study.

A communications link with the Canal Zone is being established and every effort will be made to have the meteorological data transmitted into the normal meteorological communication channels on a synoptic basis.

3. FUTURE PLANS

It is anticipated construction for the meteorology program outlined above will begin during the next dry season and the observational program can begin early in 1966 and extend through 1967. The meteorological data analysis will be completed by mid-1968.

The recommendations of the Canal Commission that follow from the feasibility studies will be the basis for future work. If it is decided to proceed with nuclear excavation along a particular route a year for planning and engineering design and two to three years of construction and drilling of emplacement holes will be required before nuclear excavation can start. During this period, an extensive meteorological service will be established along the selected route and forecasting techniques will be developed.

Some idea of the magnitude of the operation may be obtained from the preliminary estimates (1) made for excavating the Darien route (Route 17). To excavate a 1000-foot wide canal to a depth of 60 feet along a 46-mile route, 300 nuclear devices ranging in yield from 100 kilotons to 10 megatons will be used, for a total nuclear yield of 170 megatons. The depth of the emplacement holes will range from 550 to 2130 feet.

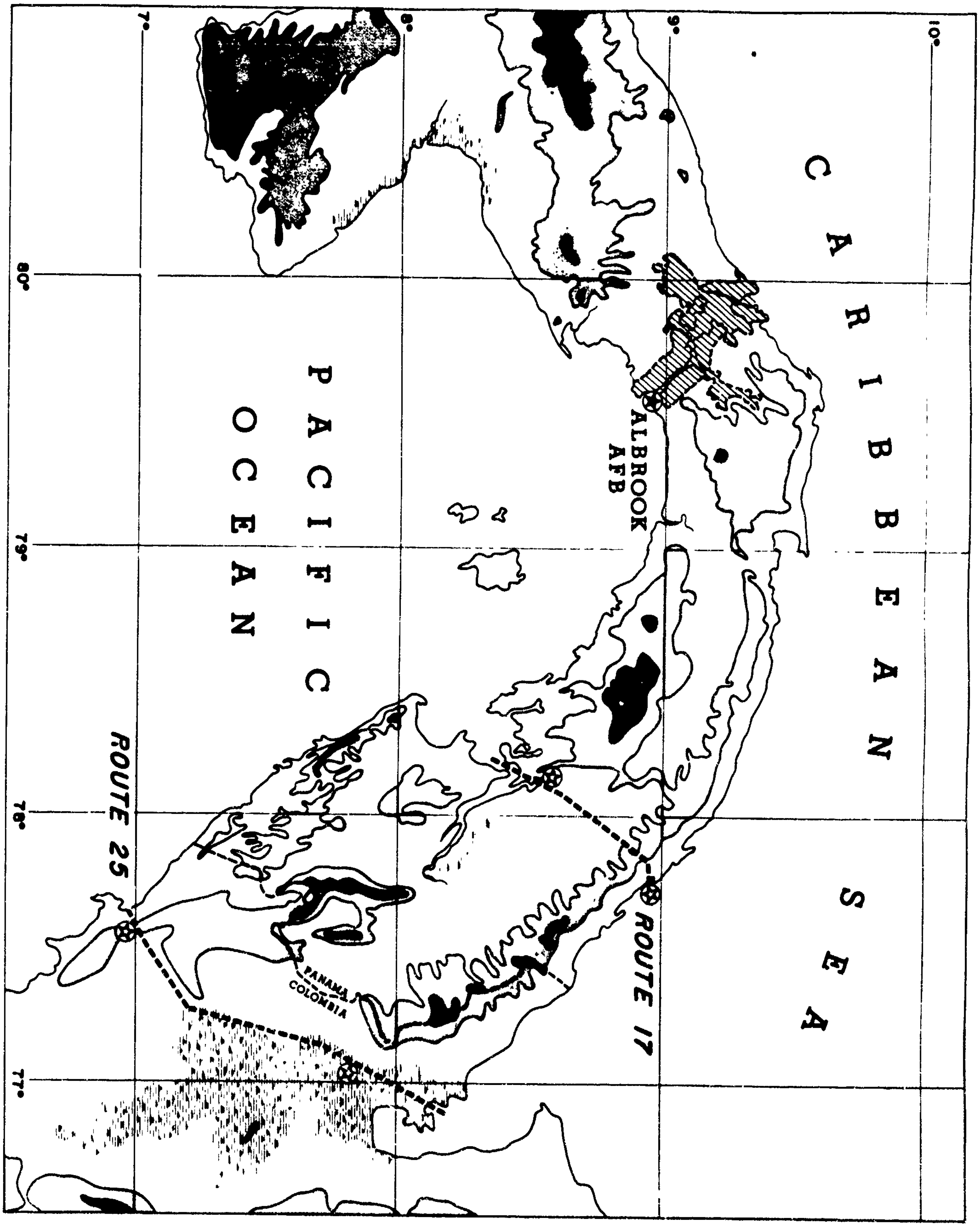
4. CONCLUSION

The feasibility of using nuclear techniques to construct a new sea-level inter-oceanic canal will depend in part on information gained from a two-year meteorological study. The program will analyze the possible hazards from fallout and rainout of radioactive debris and determine the areas to be evacuated or controlled to keep exposures within acceptable limits. The expected frequency and number of permissible firing days for the various detonations that will meet the safety criteria will be determined. The project obviously presents an opportunity for research in an area of considerable interest to meteorologists. We would welcome any suggestions from this group for specific studies to be undertaken.

Reference

Graves, E. Nuclear Excavation of a Sea-level, Isthmian Canal, Engineering With Nuclear Explosives, Proceedings of the Third Plowshare Symposium, U.S. Atomic Energy Commission, TID-7695, April 21-23, 1964.

Fig. 1. Proposed routes for the interoceanic sea-level canal and tentative sites for the two meteorological stations near each route. Elevations above 1,000 feet and 3,000 feet are shown by shading.


CARIBBEAN SEA
PACIFIC OCEAN
ALBROOK AFB
ROUTE 17
ROUTE 25
PANAMA
COLOMBIA
10°
9°
8°
7°
80°
79°
78°
77°

DISCUSSION ON LIST'S PAPER

WEICKMANN: I have doubts that your hundred-foot towers will give you much in the program unless you are specifically interested in the lower levels, because if you want to instrument the towers for wind and temperature, you are asking for a big burden.

LIST: Yes, we realize that. One of the problems with the nuclear excavation phase of it would be the very low-level winds and especially things like sea-breeze effects. We thought we would like to get some information. Now, we are trying to keep this at a minimum because the logistic problems alone of maintaining towers and establishing towers are tremendous in this area. They will probably be placed along rivers near the coast, and primarily to get wind data. Now, I somewhat agree that this is probably going to be one of the less productive phases of our program.

WHEDON: Are you planning to concentrate your observations at the spots indicated or do you plan to take observations at distances away so that you get a spread, recognizing that along the Isthmus you have very marked differences as you trafel from one end to the other?

LIST: We are faced with a logistics problem. This in parts is an impenetrable jungle and, in fact, the construction phase in itself is going to be a difficult one. That's the reason that we think we will have to rely so much on constant-level-balloon tetroon-type observations to try and outline some of the mesoscale eddies and use radar weather surveillance techniques to study precipitation patterns in the area. It would be very nice if we could move up and down the Isthmus and mount a much larger program. It may turn out that we will be able to after some preliminary construction. The Corps of Engineers intends, during this two-year period, to construct some trails and roadways along the proposed routes. In this case we will try to take advantage of any such construction to get further inland but in the main our stations are near each coast. Another problem that I haven't mentioned, and one that bothers me, is the fact that two years of study may not be two representative years, or may not give us good information as to what we can expect in the construction phase some six or seven or eight years hence. This is one of the limitations we will have to live with. We are trying to gather all the climatological data we can in that general area to see what the variability is from year to year, but this has turned out to be mainly a few rain gages that the Pan American Airways had in that area in the 1940's.

LA SEUR: Perhaps you should arrange this so that the observations and the construction come five years apart.

FREEMAN: I would like to know what consideration you have given to either ships or aircraft as platforms for releasing your tetroons.

LIST: Well, ships we have given some consideration to, especially since the Corps of Engineers apparently is going to have some water transportation available for us in association with the southernmost of those two routes, the one in Columbia. Now, it may be that we would be better off using an aircraft equipped with a doppler radar and make some sort of a rather intensive aircraft investigation program, but this turns into money. This is always a problem in programs like that and tetroons are much cheaper. So, again our budget isn't that fixed, I suppose, and there is a possibility that we will get more than we think we will. In that case, we will try and turn to more sophisticated techniques. However, my experience is that you usually get less than you think you will, not more.

COBB: This is only one comment from experience working in Western Panama. It will definitely be less sophisticated but sometimes more practical if you are interested in studying the sea breeze, by taking old tires that have been used or barrels of oil and setting them off during the morning on the coast and then by photometric methods you can check the outflow, the land breeze, the exact time it starts inland at the different levels, and such. This was very successful for us. You might think about this. I think it will be better in certain cases than trying 100-ft. towers.

LIST: Thank you for the suggestion. I would like to get together with you and see what you have done in this line.

A SURVEY OF EXTREMELY HEAVY AND PROLONGED PERIODS OF HOURLY RAINFALL, TORNADOES, FUNNEL CLOUDS, WATERSPOUTS AND HAIL IN SOUTH FLORIDA*

by
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ABSTRACT

Twenty-one years of weather observations are used to describe the climatology of extremely heavy hourly rainfalls ($\geq$2 inches), durations of hourly rainfall of moderate intensity or greater (6 or more continuous hours with no hour receiving <0.11 inches), tornadoes, funnel clouds over land, funnel clouds over water, waterspouts and hail in the South Florida area, particularly in the vicinity of Miami. All of these phenomena occur mostly during the moist season, a period when the area is predominantly under tropical influences. Fifty-five case studies involving these phenomena during the months of June and September in the six-year period 1957-1962 are examined in detail in order to determine the type of weather features and the magnitude of meteorological parameters attending their occurrence. Preliminary three-dimensional synoptic models are also presented. Comments are made with reference to triggering mechanisms aloft.

1.0 INTRODUCTION

The purpose of this survey is to first describe the climatology of several weather phenomena rarely observed in a subtropical region, and then critically examine some of those during the season when the region is predominantly under tropical influences. Upon completion of the overall study, the procedure will be to isolate and describe those that occurred in a non-hurricane tropical environment. The end product is to strive for models of small-scale systems and to improve the short-range forecast of these phenomena in such an environment.

The reason for this approach is based on the fact that much of the data in tropical regions is unsuited for critical analysis on a scale small enough to define the systems producing the phenomena of interest. The problem is not only sparsity of data, but accuracy. Perhaps one of the best suited regions for a study of this type is that of South Florida. Except for certain Bahamian stations, not only are surface observations reasonably plentiful and accurate in that general area, but upper-air observations are also. This region offers a variety of other data, and the terrain is relatively flat, free from orographic effects.

Improving the short-range forecast of rare weather phenomena in a tropical environment requires that more must be known about small-scale systems and patterns such as perturbations in the easterlies - features which are of special interest to us. It cannot be overemphasized that the data problem is a tremendous handicap for those researchers who are seeking solutions to the forecast dilemma. However, there are other handicaps as well. Some of the operational analyses in tropical areas are essentially useless or misleading in that not enough attention is paid to detail either because of training, time, directives from higher echelons, interest, or general feeling that one does not know what to do with a feature when he analyzes it. (It is fairly well proven that the forecast is no better than the analysis). Thus, critical analysis is an essential ingredient of this paper. Significant improvement of the short-range forecast in the tropics is likely to require this approach.

* This research was conducted under U.S. Army Electronics Laboratories Contract No. DA 36-039 SC-89111

** Contribution No. 629 from the Marine Laboratory, Institute of Marine Science, University of Miami. Published in the Proceedings of the 1965 Army Conference on Tropical Meteorology, Miami Beach, Florida, 6-7 May 1965.

2.0 CLIMATOLOGY

Combined hourly rainfall data from the following seven stations were used to describe the climatology of extremely heavy hourly rainfalls (≥2 inches) and durations of hourly rainfall of moderate intensity or greater (6 or more continuous hours with no hour receiving <0.11 inches) in the South Florida area: Lignumvitae Key, Boca Raton, Hialeah, Homestead Experiment Station, Pennsuco 4 NW, North New River Canal #2, and Tamiami Trail -- 40 Mile Bend. The daily record of surface weather observations and local climatological data taken at the National Hurricane Center in Miami were used to describe the climatology of tornadoes, funnel clouds over land, funnel clouds over water, waterspouts and hail in the vicinity of Miami. Occurrences were listed for approximately the same area as that used for the rainfall portion. A summary of the maximum occurrences of these phenomena for the period 1942-1962 is shown in Table 1.

	TIMES OF MAXIMUM OCCURRENCES					NO. OF OBS. USED ALL MONTHS ALL YEARS
			HOUR ENDING (EST) BY SEASONS			
	YEAR	MONTH	ALL	MOIST	DRY	
EXTREMELY HEAVY HOURLY RAINFALLS*	1959	JUNE	1400	1300	1400	114
DURATIONS OF MODERATE HOURLY RAINFALL**	1960	SEPT	1300	1400	1300 and 1900	88
TORNADOES	1956 and 1959	JUNE	1300 and 2200	1300	1200	21
FUNNEL CLOUDS OVER LAND	1960 and 1962	JUNE	1400	1400	1300 and 1400	55
TORNADOES AND FUNNEL CLOUDS OVER LAND	1960	JUNE	1400	1400	12,13,14 and 19	76
WATERSPOUTS***	1957	MAY and JULY	0800	0800	1400	40
FUNNEL CLOUDS OVER WATER	1943,1945 1959,1960	JUNE	08,10,13 and 19	08,10,13 and 19	NONE	4
WATERSPOUTS AND FUNNEL CLOUDS OVER WATER	1957	MAY, JUNE and JULY	0800	0800	1400	44
HAIL	1957	MAY	1400	1400	1600	56

* Greatest was 5.90 inches at Boca Raton, 18 Jan. 1942, during the hour ending 1700 EST

** Longest was 15 hours at North New River Canal #2, 1 Oct. 1951, starting during the hour 1800-1900 EST

*** Although these data do not show it, waterspouts are fairly common in South Florida and probably should not be classified as a rare weather phenomenon.

TABLE 1 - TIMES OF MAXIMUM OCCURRENCES OF SEVERAL RARE WEATHER PHENOMENA IN SOUTH FLORIDA DURING THE 21-YR PERIOD 1942-1962.

Most of these phenomena occur during June except for the rainfall durations which occur mainly in September, waterspouts which occur essentially in May and July, and hail which is more frequent in May. It is interesting to note that they occur mainly during the early afternoon except for waterspouts which occur mainly during the hour ending at 0800 EST or just after sunrise. A double maximum is indicated for tornadoes with the later one occurring during the hour ending at 2200 EST. The stratification according to "moist" season (May-October) and "dry" season (November-April) shows essentially the same hours of maximum occurrence except for waterspouts which occur mainly during the early afternoon in the dry season. A plot (not shown) of the average length of durations beginning at each hour of the day indicates that the longest average length (based on more than five occurrences) is associated with rainfalls that begin at 1800 EST. The graphs from which this table was made show that hail practically never occurs at night during the moist season.

3.0 TYPICAL WEATHER FEATURES AND MAGNITUDES OF METEOROLOGICAL PARAMETERS ATTENDING RARE WEATHER PHENOMENA IN SOUTH FLORIDA

Fifty-five case studies involving rare weather phenomena in South Florida during the months of June and September in the six-year period 1957-1962 were examined in detail to determine the type of weather features and the magnitude of meteorological parameters which most frequently attended their occurrence. The events were grouped into the nearest six-hour synoptic time, and various meteorological parameters observed at Miami were noted for these times. Weather features at the surface were extracted from Miami and San Juan operational maps because of a lack of time for their reanalysis. It was noted that San Juan analyses show many more perturbations in the easterlies in the Miami area than do those from Miami, and there were obvious indications of attempting to maintain continuity. However, although the author strongly feels that more perturbations are reasonable, the tendency for San Juan to tie in a large percentage of perturbations on the ITC with perturbations in the easterlies and thence to the middle latitudes is not justifiable. Since more Miami surface maps were used, the results of this survey will show an underestimate of these features. Neither emphasized the analysis of cols at the surface and they also will be underestimated. Upper-air features were based on reanalysis of wind maps at 5000 and 20,000 ft. All map features were stratified into twenty surface categories and into twenty upper-wind patterns. Features at all three levels were coded on IBM cards such that first priority was assigned to those within 100 n. mi from Miami. If nothing significant was observed in this range, then the range interval from 100-200 n. mi was used with provision in the code for location (N, S, E, or W) from Miami. If nothing of special significance was observed in either of these range intervals, then the best general description of the situation within 200 n. mi from Miami was coded.

A summary of the most common features observed at the surface, 850 mb and 500 mb, along with values of various meteorological parameters aloft is shown in Table 2. The range of observed values is indicated by the upper and lower numbers in each data box, with the mean value in the center. The data in each box are <u>independent</u> of other data. Inspection of Table 2 indicates a feature referred to as a "lip". This will be described in a later section. Temperature, moisture and radar refractivity are expressed in terms of anomalies from Miami mean monthly data by Jordan (1957) and Bean et al (1960). Radar refractivity data were included primarily because of their dependence on moisture and temperature. The temperature-dew point spread at 500 mb was included in the stability section to see if perhaps it would suggest a method of modifying the Showalter Index (which is not of much use in this area) to account for humidity at this level. The spread of the mean June data is 9 degrees and 8 degrees in September.

	RAINFALL ≥ 2.00"/hr	DURATIONS ≥ 6 hrs	TORNADOES	F.C. OVER LAND	WATER-SPOUTS	F.C. OVER WATER	HAIL
SFC WX (# OF SYNOPTIC TIMES)	RIDGE OR ANTICYC. FLOW (8)	CYCLONIC PERT IN EASTERLIES (4)	S.E. TRADES (2)	CYCLONIC PERT. IN EASTERLIES (6)	S.E. TRADES (3)	RIDGE AND PERT. IN EASTERLIES (2)	FLAT GRAD. (2)
850 MB WX (# OF SYNOPTIC TIMES)	CONFL. (6)	VORTEX IN EASTERLIES (4)	COL.-DIFLU. (3)	CONFLU. (4)	LIP.-CONFLU (2)	ANTICYC. FLOW and CONFLU. (2)	COL.-CONFLU. (3)
850 MB WIND DIR. AND SPEED	29 30 18/06 05 05	27 70 16/18 08 05	17 25 16/05 31 05	24 15 18/05 09 05	29 15 14/05 09 05	24 20 19/12 16 10	21 10 28/03 07 05
850 MB T ANOMALY	2 0 -1	3 0 -2	0 -1 -2	1 0 -2	0 -1 -1	-1 -1 -1	1 0 -1
850 MB T_d ANOMALY	4 1 -2	4 2 -1	2 0 -3	1 0 -3	2 -1 -3	-4 -4 M	1 0 -1
850 MB REFRAC. ANOMALY	21 6 -5	21 11 0	11 5 -9	7 1 -10	9 -1 -8	-11 -11 M	8 3 -1
500 MB WX (# OF SYNOPTIC TIMES)	COL.-DIFLU. (5)	VORTEX IN EASTERLIES (5)	CONFLU. (2)	CONFLU. (2)	LIP.-CONFLU (2)	CONFLU. and TROUGH IN WESTERLIES (2)	CONFLU. (2)
500 MB WIND DIR. AND SPEED	02 30 21/06 11 05	24 70 16/18 03 05	34 20 23/04 11 10	02 25 28/04 10 05	21 20 29/05 09 05	27 10 26/10 25 10	A 20 L 26/07 L 05
500 MB T ANOMALY	2 1 -2	5 1 -1	2 1 0	2 0 -1	2 0 -1	1 0 -1	1 -1 -2
500 MB T_d ANOMALY	9 3 -5	11 2 -10	9 3 -7	9 -1 -10	6 -1 -7	4 -1 -5	6 -2 -10
500 MB REFRAC. ANOMALY	8 3 -1	12 4 -5	8 3 -3	8 2 -4	7 1 -3	4 2 -1	6 0 -4
SHOWALTER INDEX	3 0 -3	4 1 -3	3 2 1	3 1 0	3 1 -1	5 5 5	1 -1 -2
$T-T_d$ at 500 MB	13 6 [illegible]	19 8 2	16 8 2	18 9 2	16 10 2	13 10 6	17 10 3
SHEAR (850-500)	A 26 L 01/03 L 05	A 37 L 33/04 L 05	33 23 29/05 11 10	23 22 30/07 02 04	A 16 L 29/06 L 05	A 25 L 32/12 L 05	A 11 L 18/03 L 04
RATIO OF VEERING/BACKING	7/7	4/5	1/2	9/3	2/3	2/0	3/4
TOTAL # OF SYNOPTIC TIMES	15	10	4	13	7	2	8

TABLE 2 - SUMMARY OF THE MOST COMMON FEATURES AND MAGNITUDES OF METEOROLOGICAL PARAMETERS ATTENDING RARE WEATHER PHENOMENA IN SOUTH FLORIDA. Independent data are listed for each phenomenon, therefore, they should not be thought of as occurring simultaneously.

Inferences which can be made from these June and September data are as follows:

1) Rainfall durations and funnel clouds over land occur mostly with perturbations in the easterlies. The other rare weather phenomena occur in unsuspicious areas at the surface.

2) With the exception of tornadoes and rainfall durations, they occur with confluence aloft. The confluence may be associated with a normal asymptote of confluence, a col, or a "lip".

3) Low-level difluence is preferred for tornadoes to occur.

4) Rainfall durations occur with vorticies aloft.

5) The mean low-level wind is southerly for all except hail.

6) The mean 500 mb wind is westerly for all except rainfall durations.

7) Mean temperatures are preferred at 850 mb except for tornadoes, waterspouts and funnel clouds over water which prefer slightly cooler air at this level.

8) Mean moisture is preferred at 850 mb for tornadoes, funnel clouds over land, and hail. Waterspouts and funnel clouds over water prefer somewhat dryer air, and both types of rain prefer air of greater moisture at this level.

9) At 500 mb, mean temperatures are preferred for funnel clouds over land, waterspouts and funnel clouds over water, whereas slightly warmer air is preferred for both types of rain and tornadoes. Slightly cooler air is preferred for hail.

10) Greater than mean moisture is preferred at 500 mb for both rains and tornadoes, whereas slightly dryer than mean moisture is preferred for the others.

11) If one assumes that the spread of the mean temperature data is representative of the mean temperature-dew point spread, then rainfall durations, tornadoes, and funnel clouds over land occur with normal humidity at 500 mb. Extremely heavy hourly rainfalls occur with greater humidity, and waterspouts, funnel clouds over water and hail occur with less humidity at this level. (Modification of the Showalter Index is not clear from these data).

12) Greater than normal radar refractivity is indicated at both levels except at the lower level for waterspouts and funnel clouds over water.

13) The mean shear has essentially no easterly component. Except for funnel clouds over land, neither wind "veering" or "backing" with height is preferred.

Location of the high-level jet stream was not tabulated, however, the only strong winds noted at 850 and 500 mb were in association with Hurricane Donna. Two of the fifty-five case studies were rainfall duration situations produced by Hurricane Donna. High-level divergence was not investigated either because of a lack of wind information above 25,000 ft at pibal times. We may include both of these parameters later using rawin data.

4.0 PRELIMINARY MODELS

The data in the previous section were independent summaries without regard for other data or levels. These data were screened to determine what preferred three-dimensional combinations of features and parameters occurred simultaneously to produce an environment favorable for the formation of the rare weather phenomena. The resulting models are presented in Table 3. The data sample is relatively small because of the imposed restrictions, but these preliminary models should be useful until others are developed.

	RAINFALL ≥ 2.00"/hr	DURATION ≥ 6 hrs	TORNADOES	F.C. OVER LAND	WATER SPOUTS	F.C. OVER WATER	HAIL
SFC WX	RIDGE AXIS IN AREA S.E.TRADES	CYCLONIC PERT IN EASTERLIES	S.E.TRADES NEUTRAL PT.	RIDGE AXIS IN AREA,NEUTRAL PT., PERT. IN EASTER.	S.E. TRADES	RIDGE AXIS IN AREA	FLAT GRAD.,NEUTRAL PT. OR RIDGE AXIS IN AREA
850 MB WX (ORDER PREFERRED)	CONFLUENCE (ASYMP, LIP, COL)	VORTEX IN EASTERLIES	DIFLUENCE (COL, LIP)	CONFLUENCE (ASYMP, COL)	CONFLUENCE (LIP, COL, ASYMP)	ANTICYC. FLOW	CONFLUENCE (COL, LIP, ASYMP)
850 MB WIND DIR.-SPEED	SW/10	SW/5-20	SE-S-NW/10-25	SE-SW/5-15	E-SW-NW/5-15	SE/20	SW-NW-NE/5-10
850 MB T ANOMALY	-1 to 2	-2 to -1	0	0	-1 to 0	-1	-1 to 1
850 MB T_d ANOMALY	-2 to 1	0 to 1	-3 to 2	-1 to 1	-2 to 2	-4	0 to 1
850 MB REFRAC. ANOMALY	-5 to 7	4 to 8	-9 to 11	-3 to 7	-5 to 11	-11	-1 to 8
500 MB WX (ORDER PREFERRED)	DIFLUENCE (COL)	VORTEX IN EASTERLIES, WESTERLIES	CONFLUENCE (ASYMP, LIP)	CONFLUENCE (ASYMP, COL, LIP)	CONFLUENCE (LIP, COL, ASYMP)	CONFLUENCE (ASYMP)	CONFLUENCE (ASYMP, LIP, COL)
500 MB WIND DIR. AND SPEED	SE/05	S-SW/10-20	SE-SW/10-20	SW/5-10	NE/10 or SW/5-20	W/10	SE-SW-NW/10-20
500 MB T ANOMALY	0 to 2	0 to 1	0 to 2	0 to 2	-1 to 2	1	-2 to 1
500 MB T_d ANOMALY	1 to 8	-10 to 5	-7 to 9	-10 to 9	-6 to 6	4	-10 to -1
500 MB REFRAC. ANOMALY	3 to 7	-5 to 7	-3 to 8	-4 to 8	-3 to 7	4	-3 to 2
SHOWALTER INDEX	0 to 3	0 to 3	1 to 3	0 to 3	-1 to 2	5	-2 to 1
$T-T_d$ at 500 MB	3 to 7	3 to 19	2 to 16	2 to 18	2 to 15	6	7 to 16
SHEAR (850-500)	NE/10-15	E or W/5	SE-S-NW/10-25	W-NE/5	NE-SE-SW/5-15	NW/25	SE-SW-NW/10
VEERING OR BACKING	V or B	V or B	V or B	SLIGHT PREF. FOR VEERING	BACKING PREFERRED	VEERING	SLIGHT PREF. FOR VEERING
# OF SYNOPTIC TIMES/TOTAL #	4/15	2*/10	3/4	4/13	4/7	1/2	4/8

*NON-HURRICANE SITUATIONS

TABLE 3 PRELIMINARY THREE-DIMENSIONAL MODELS OF THE SYNOPTIC ENVIRONMENT FAVORABLE FOR THE FORMATION OF RARE WEATHER PHENOMENA IN SOUTH FLORIDA. Dependent data are listed for each phenomenon; therefore, they should occur, simultaneously to produce the phenomenon.

5.0 TRIGGERING MECHANISMS ALOFT

The streamline analyses of the upper-wind charts were made with as little bias as possible. An effort was made to draw asymptotes only where they were relatively obvious, instead of placing them according to preconceived notions. In doing this, asymptotes of confluence either separately or associated with a col still appeared in the area when the rare weather phenomena were observed. These in themselves undoubtedly provided triggering actions.

In addition, it was noted that a "lip" (as mentioned earlier) was often evident in the area during times when rare weather phenomena were observed. The feature referred to is illustrated by dashed lines in Figure 1. The terms "lip" and "diamond" are used for lack of a better description of these patterns. For this study, "lips" and "diamonds" were both included in the general category of "lips", and the only distinction made was whether the confluent or the difluent portion was nearest Miami. As shown in Figure 1, this feature is logically associated with wave or cellular development brought about by differential movement of two wave trains at different latitudes as described by Namias and Clapp (1951). Although Namias and Clapp proposed this for an index cycle involving many degrees of latitude, it is reasonable to apply this on a much smaller scale, in the westerlies or easterlies. The net result is that the so-called "lips" and "diamonds" are associated with wave or cellular development. Thus, there may be some connection between genesis and rare weather phenomena in South Florida. The reverse cycle would be associated with decay which would not be as likely to trigger such phenomena.

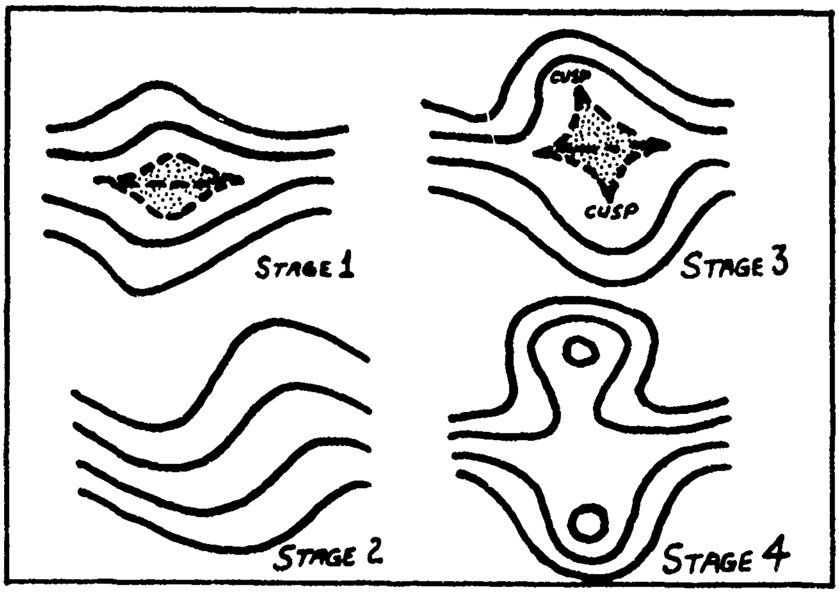

FIG. 1 - SCHEMATIC REPRESENTATION OF SUCCESSIVE CIRCULATION PATTERNS ALOFT DURING AN INDEX CYCLE (After Namias and Clapp 1951). The dashed patterns by Gerrish are termed "lip" in Stage 1 and "diamond" in Stage 3.

6.0 COMPARISON WITH OTHER STUDIES

While it is recognized that there are different types of tornadoes, hailstorms, etc., each with somewhat different "personalities", it is also reasonable to expect variation with latitude and geographical location as well. Frisby (1964), Hiser and et al (1958 and 1962), Miller (1959), Neumann (1963) and Sourbeer et al (1961) have written on specific cases of hail, heavy rain, and tornadoes in the South Florida area. However, their approaches were significantly different such that no comparisons can be made at this writing. Comparisons can be made as other parameters are incorporated in the work.

7.0 CONCLUDING REMARKS

Oddly enough the weather phenomena discussed in this paper are rarely observed in tropical regions, but when they occur, it is mostly during the moist season in South Florida, a time when this region is predominantly under tropical influences. Some occur with tropical systems, some with mid-latitude systems (mostly aloft) which penetrate this region then. They occur under nearly normal tropical conditions in the lower troposphere except for the 500 mb dew point temperature which may vary substantially toward both higher and lower moisture conditions at this level. A cursory look at the models reflects that singularly the three-dimensional combination of features in the wind field is more important than the other parameters for prediction of these events to occur.

It is evident from this survey that cols both at the surface and aloft play a significant role in the production of rare weather phenomena in South Florida and their presence should not be ignored. The evidence here supports the importance of asymptotes of confluence aloft in the production of these phenomena. The presence of "lips" perhaps offers additional insight into the triggering mechanism aloft.

Although surface maps were not reanalyzed for this study, previous experience indicates that the reanalysis would show many small-scale perturbations in the easterlies in regions of the trades and sometimes where ridges exist. Since these regions in this survey were typically associated with rare weather events, it is suggested that these perturbations are important in that they either provide the necessary organization of convection, the triggering mechanism, or both.

8.0 REFERENCES

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DISCUSSION ON GERRISH'S PAPER

FREEMAN: Is that last picture still on the opaque projector? Where is the front?

GERRISH: It's in South Florida, the southern tip of South Florida.

FREEMAN: That is something like a blocking pattern, as you mentioned. The blocking pattern, of course, can be obtained from a strictly barotropic flow, just based on the variation of the Coriolis parameter, but you can get the same kind of variation in vorticity with Y, if you want to call it that, with a north-south direction by means of convergence and divergence under a front. In other words, if you have a sloping frontal surface, and in this particular case when you go to the south, you would undergo divergence and increase your anticyclonic vorticity. When you go to the north, you would undergo convergence and increase your cyclonic vorticity. You would get exactly the same pattern or a small scale depending on the slope of the front, the scale being dependent on the slope of the front. As you get on a planetary scale, this is due to variation of Coriolis parameter. This looks like to me exactly what you have found in your particular situation, that in order to have these patterns on a mesoscale, you have to have a fixed convergence and divergence producing mechanism. A stable frontal surface would be one, there may be others; ground, mountains, or something like that could serve the same purpose. It does look to me that you have by study of the data isolated some cases in which this convergence and divergence is taking place to bring about the same kind of pattern that you have on a planetary scale.

GERRISH: In the absence of fronts and in the presence of what I prefer to work with more and more, namely waves or perturbation situations in the easterlies, would you care to comment on the occurrence of what I have so-called "lips" and that sort of thing in the area under those situations where trade-wind inversions may be destroyed and there are no frontal systems aloft?

FREEMAN: I'll make two comments. One is my customary comment to that statement, and that is I think it's rather likely that you have a stable atmosphere with sloping lines of potential temperature. Even if you don't have a formal inversion, you have a sloping stability surface. Then, I would add to that, that at the moment I can think of no other mechanism to get the vorticity changes that you get, other than convergence or divergence on that scale. It seems to me it is much too small a scale for the beta term to bring about the changes in vorticity that you have there, so that my first guess is that you have divergence to the south and convergence to the north when you have those patterns. Moving northward or moving to the right of the main flow brings about convergence and moving to the left brings about divergence. Now, right now I can think of no other way for that to be brought about. There may be other ways, but I am fairly sure that this convergence and divergence pattern is taking place. It's hard to imagine anything else.

BARGMAN: I think, in fact, we have so much given to us in the tables that we will require a certain amount of digestion before we can attack them.

GERRISH: That's true. I was essentially trying to test this latter mechanism on you to get your reaction. I was aware that if you could see the tables, you couldn't digest them, so I tried to hurriedly skip over them. I mainly wanted to test your reaction on these differential wave trains moving on a mesoscale and see if you agreed with my interpretation of these patterns that I observed.

GOLDMAN: I seem to recall a seminar given by C.E. Palmer in Chicago around 1962 where he develops hurricanes from this same type of pattern. Starting from a perturbation in the easterlies, well not really the easterlies but the equatorial trough, he developed a cusp, the point singularity and the line singularities, and then right into a nice hurricane. Are you discounting this, saying that this is a system for all tropical disturbances?

GERRISH: I don't think I'm saying either one. I'm certainly not discounting that because not enough really is known about the development of hurricanes but what I am trying to say is that I'm not sure what degree of development is apparently important at least for these types of rare weather phenomena to occur. I think that the formation would be more preferred than the decay. Certainly, this cycle has to go through a decay period, otherwise, you'll never get any change and I think the formation period would be more preferred than the decay portion

KOTESWARAM: With regard to the reactions that you are seeking from us regarding flow patterns that you have associated with these phonomena, I may say that in India, too, we have been observing for quite a long time the existance of such rather serious storms and thunderstorms and the like, in relation with these lengths of convergence and col periods which you have mentioned. About the year 1944, a meteorologist called Dr. Sen was trying to analyze it in terms of Karman's work on vortex streets. He put in the axis of dilatation and axis of compression, so at the end of these axis of dilatation, he was always looking to it for the existence or the development of weather. Well, that was good enough for quite a long time, but later on when we were looking a bit more closely into these things, it was not always the case that the low-level feature above could delineate the weather. One has to go into the upper-level features, too, and therefore, we now always look to the existence of the low-level feature, like the convergence in an axis of dilatation or an axis of confluence topped by an upper-level divergence in order to give an outbreak of thunderstorms.

TROPICAL RAINFALL PATTERNS AND ASSOCIATED MESO-SCALE SYSTEMS

Walter K. Henry and John F. Griffiths
Texas A&M University

Abstract

The month of October 1954 throughout the Pacific coast area of Panama and Costa Rica was very wet having in excess of 1800 mm of rainfall. This singularity is described and some suggestions offered as to the cause.

The rainy and wet seasons, identified by the method outlined in earlier Reports, are shown for a selection of stations in Central America. These illustrate the various patterns obtained. The maps of standard deviation of rainfall are discussed and the correlation between rainfall amount and standard deviation is obtained. A preliminary report on the use of harmonic analysis is also included.

Meso-Scale Rainfall Analysis - W. K. Henry

Mr. Chairman, Ladies and Gentlemen:

During the past year we have been investigating the rainfall patterns in Central America and doing some initial work in South America. The data I obtained in Ecuador last summer is being used in two theses. One which Mr. Guest is writing is comparing one small area in Ecuador with an area of about the same size in Central Texas. Each has about thirty rain gages in an area of 10 by 20 miles. Initially, the most noticeable difference is the diameter of the daily one-inch isohyet. In Texas the area covered has about twice the diameter, indicating larger cells, or clusters of cells. It appears that the rainfall in Ecuador is from one-cell storms.

Mr. Rumley is making a study of rainfall and elevation in Ecuador. Variations of rainfall are apparent but do not seem to relate very well to elevation.

Mr. Barnard has been using our Central American data to study monthly rainfall variations in an areal distribution. He has had some success in relating areas. Also the computations have indicated some unexpected results. His work will be beneficial to many national agricultural groups because it gives such a wide coverage of the area.

The meso-scale analysis has progressed throughout the year, with analysis of daily rainfall in Nicaragua and Panama-Costa Rica. The meso systems show up well but no systematic movement has been detected. Also mean rainfall maps were constructed for Central

America. These are a side effort but since such a large quantity of data were available it seemed to be appropriate.

Today, I wish to discuss a singularity in the monthly rainfall which occurred in the area of the Panama-Costa Rica border. The month is October of 1954. Many of you may recall Hurricane Hazel occurred that month, but Hazel did not extend into this area directly.

The average rainfall for October is variable across this area and ranges between 500 to 800 mm (20 to 30 inches). During the month of October 1954 many of these stations had in excess of 1600 mm which was 2 to 3 times the mean. Figure 1 shows the rainfall amounts for October 1954. The two areas of maximum amounts can be identified. There were extensive floods from these rains. Also the minimum area of just under 1000 mm should be noted. Figure 2 shows the percent of the mean for the month. Here the excess shows in an area in Panama which had more than three hundred percent of the mean. The rainfall total is more than three standard deviations from the mean using either the standard deviation of the mean value or of the square root means and their standard deviation. (1) Thus the chances of this much rainfall occurring, is small, less than once in 200 years, but it did happen in 1954.

Let us look at the daily rainfall which makes the monthly totals. The data for five stations is shown in Table I. These five stations are scattered throughout the area. An examination of the other stations indicate that they are representative of all in regards to amounts and days the heavy rains fell. The locations of these stations are indicated by stars on all the figures and Figure 2 identifies them by name.

From Table I we note that some rain fell most of the days of the month, which is not unusual for this time of the year. On the 12th, heavy rains fell at these five stations and over all the area. Again on the 19th heavy rains fell. The rainfall areas seemed to be moving from southeast to northwest during the 11 and 12, but from the northwest to southeast on the 19, 20 and 21.

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TABLE I

Daily rainfall amounts from selected stations in Costa Rica and Panama, October 1954. Inches.

Day	Armuelles	Farm 15 (Sierpe)	Farm 97 (Coto Norte)	Heredia (Esquinas)	Farm 1 (Palmar)
1	5.94	.75	.92	.50	1.42
2	1.08	.80	1.20	.85	1.00
3	.60	.85	.40	.50	1.25
4	.16	.10	.74	.05	.30
5	.23	1.05	1.35	.52	1.28
6	.43	1.25	.20	1.50	.63
7	.18	2.70	1.38	1.28	1.76
8	4.23	9.10	2.30	3.30	4.23
9	1.10	0.00	2.97	0.00--	.25
10	2.20	3.50	1.80	.97	2.20
11	5.95	6.00	2.10	3.50	6.30
12	6.90	12.50	5.20	3.80	5.15
13	2.08	6.50	1.16	2.85	3.10
14	.32	.90	.15	.95	1.50
15	.12	.85	1.12	1.80	1.15
16	1.00	1.70	1.47	2.50	1.30
17	3.23	3.50	1.07	1.60	1.83
18	1.36	1.50	6.64	4.10	1.95
19	1.00	5.20	11.00	7.20	6.20
20	1.94	3.55	3.50	2.65	2.03
21	7.90	1.00	1.72	1.90	2.05
22	9.34*	.10	.70	.05	.05
23	3.15	.05	.51	.75	.25
24	1.50	3.25	1.10	1.90	1.70
25	.22	.15	1.60	3.19	.05
26	1.48	3.50	.91	.65	.16
27	.74	.32	.13	.18	.11
28	.26	1.20	.58	2.20	1.15
29	.12	.25	.45	.50	.60
30	.20	.60	.42	.40	1.75
31	0.00--	5.00	2.53	1.90	1.28

Figure 3 shows the rainfall for the 12th. From the limited data available (we hope to locate more) it appears that a tropical disturbance is moving along the coast causing the heavy rainfall. Figure 4 shows the rainfall on the 19th. This heavy rainfall moved to the southeast during the next two days. Again synoptic data is limited, but the polar front was carried far to the south by Hurricane Hazel, and by continuity the remnants of the front would be moving through this area by this time. There is a wind shift and a slight temperature drop, of about 2°F in the daily means. Armuelles received its most rain on the 21st and 22nd as this weak discontinuity worked toward the south. This discontinuity could hardly be called a polar front, but by any name, it is associated with a lot of rain.

Plans for the immediate future.

1. Extend the general investigation into South America. Some data has already been obtained in these areas and preliminary investigation started.

2. Identify the meso-scale systems and their behavior in different seasons and geographic areas.

3. Investigate more of the singularities which are being found.

In terms of extended plans, more effort is needed in identifying synoptic situations. However, this requires a type of data which is very limited.

Are there any questions?

It is my pleasure to introduce my pardner, Mr. Griffiths who will describe some of his work.

Reference

1. W. K. Henry, J. F. Griffiths, and G. Cobb, Research on Tropical Rainfall Patterns and Associated Meso-Scale Systems, Report Nr. 7, January 15, 1965, Texas A&M University.

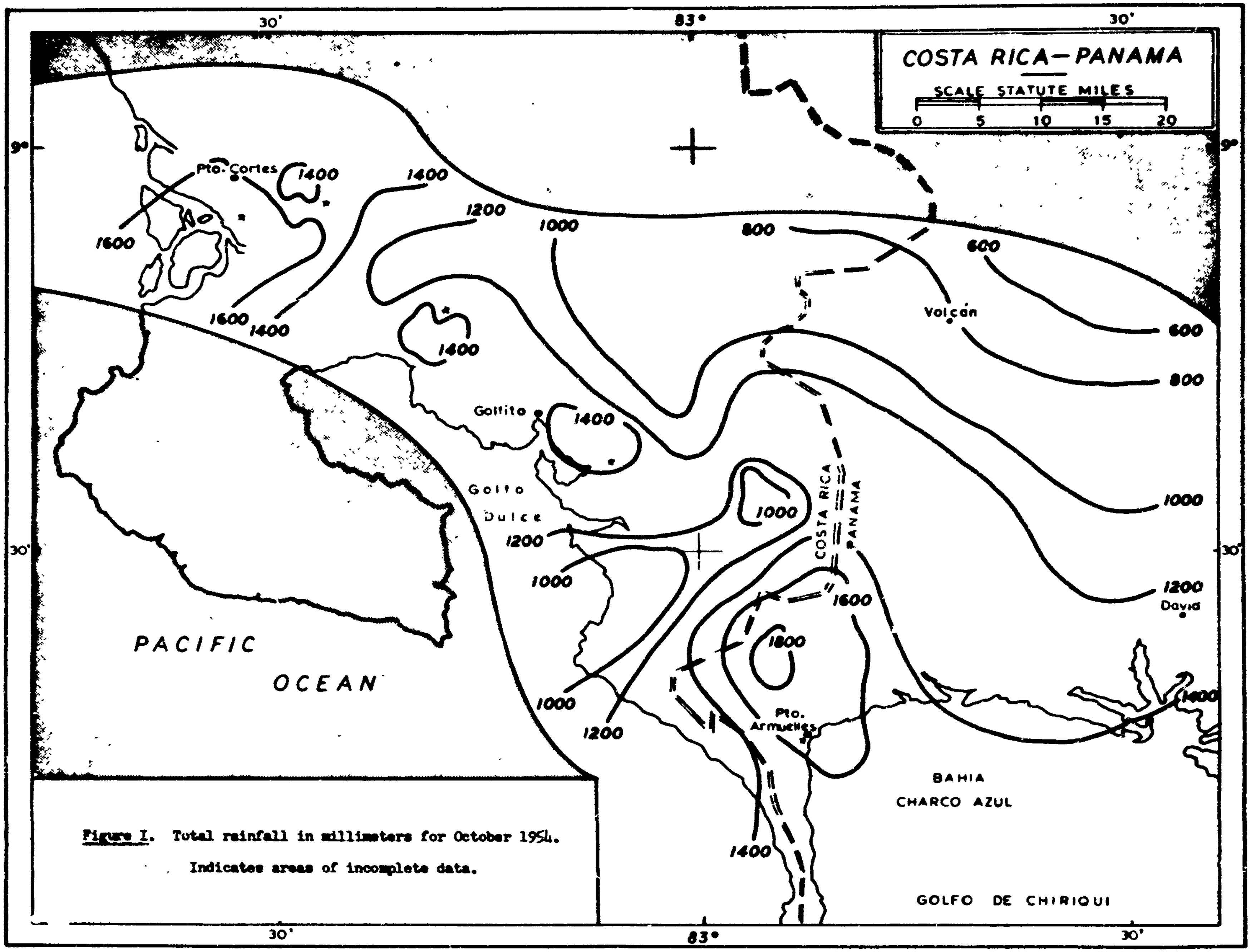


Figure I. Total rainfall in millimeters for October 1954.

* Indicates areas of incomplete data.

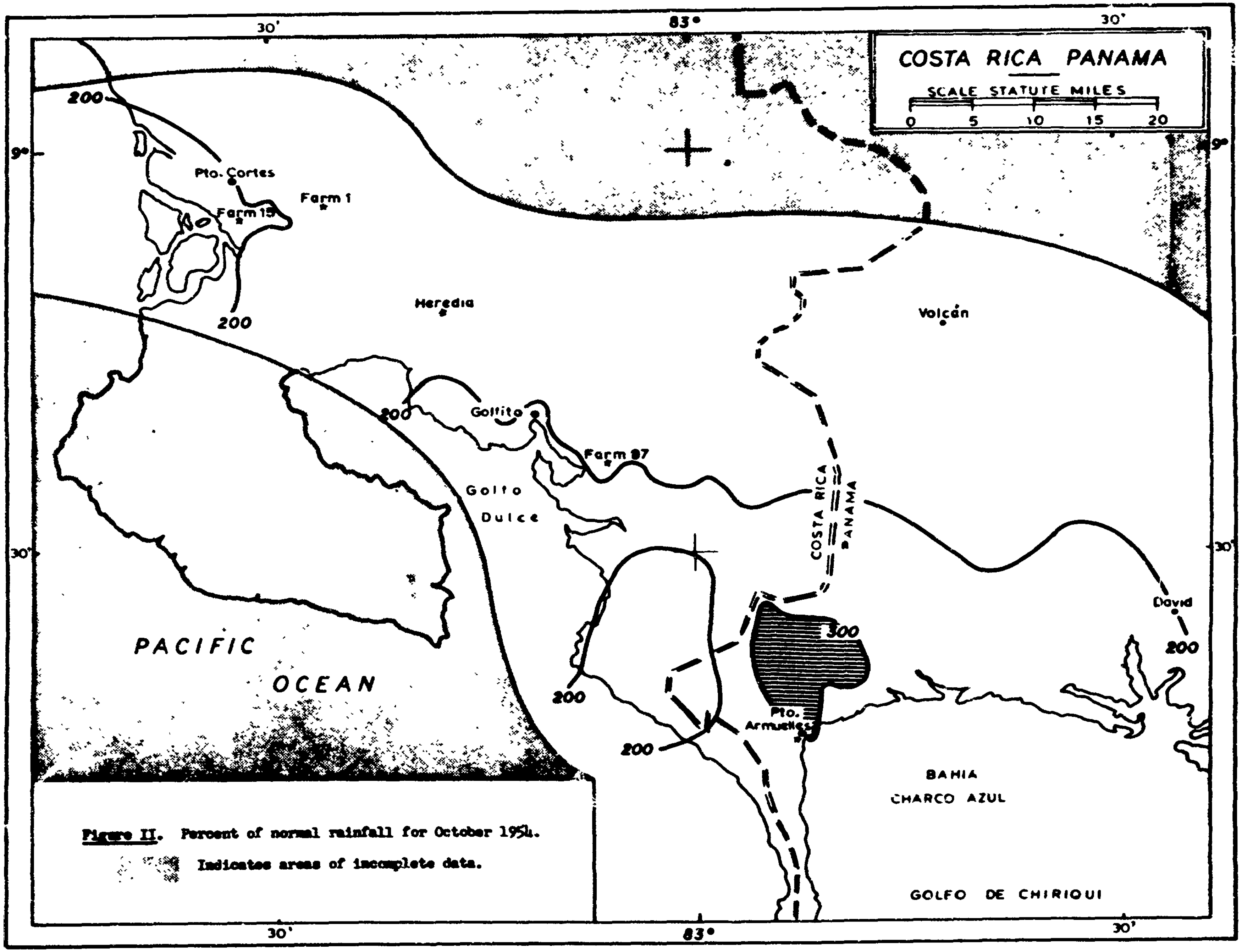


Figure II. Percent of normal rainfall for October 1954.

Indicates areas of incomplete data.

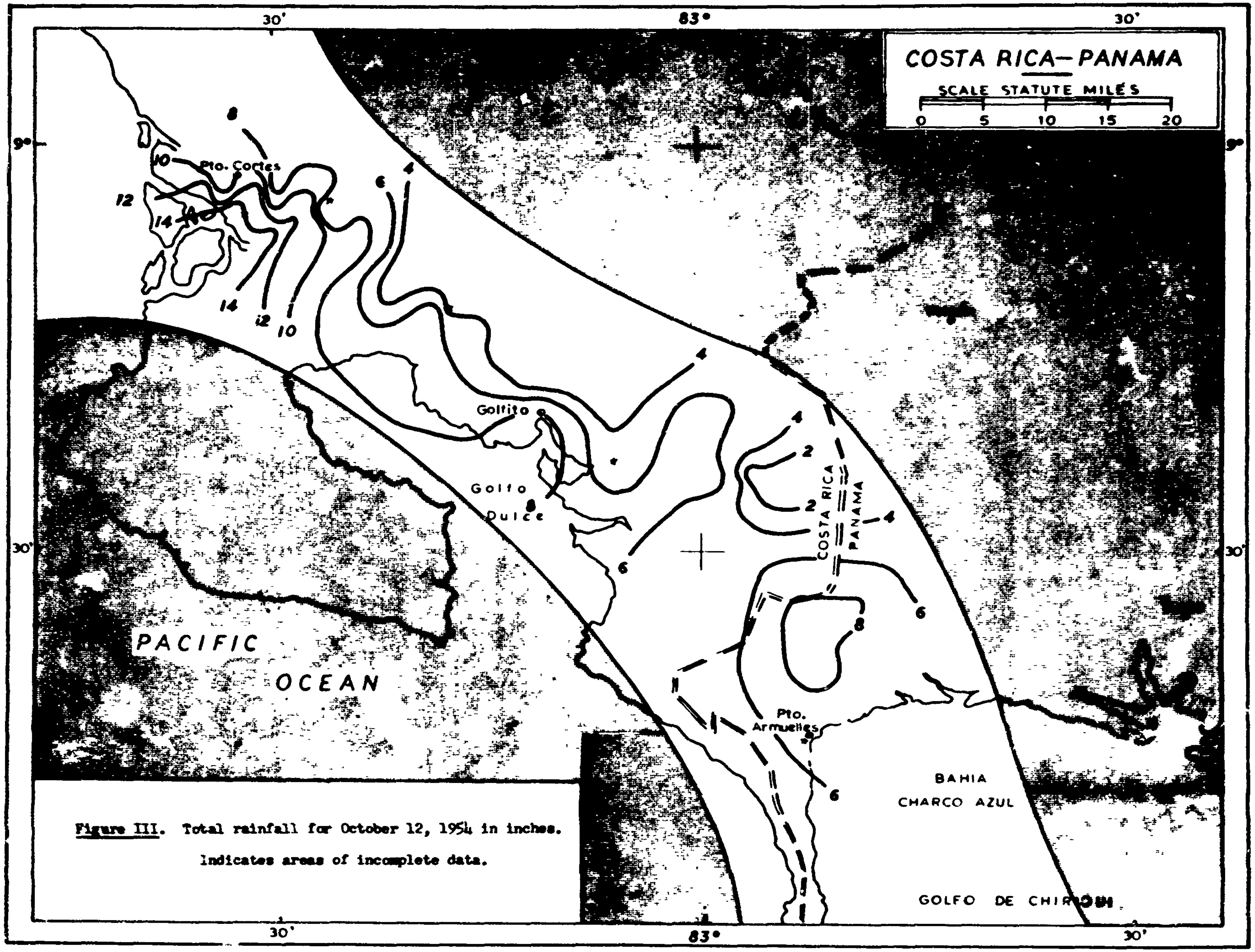


Figure III. Total rainfall for October 12, 1954 in inches.

Indicates areas of incomplete data.

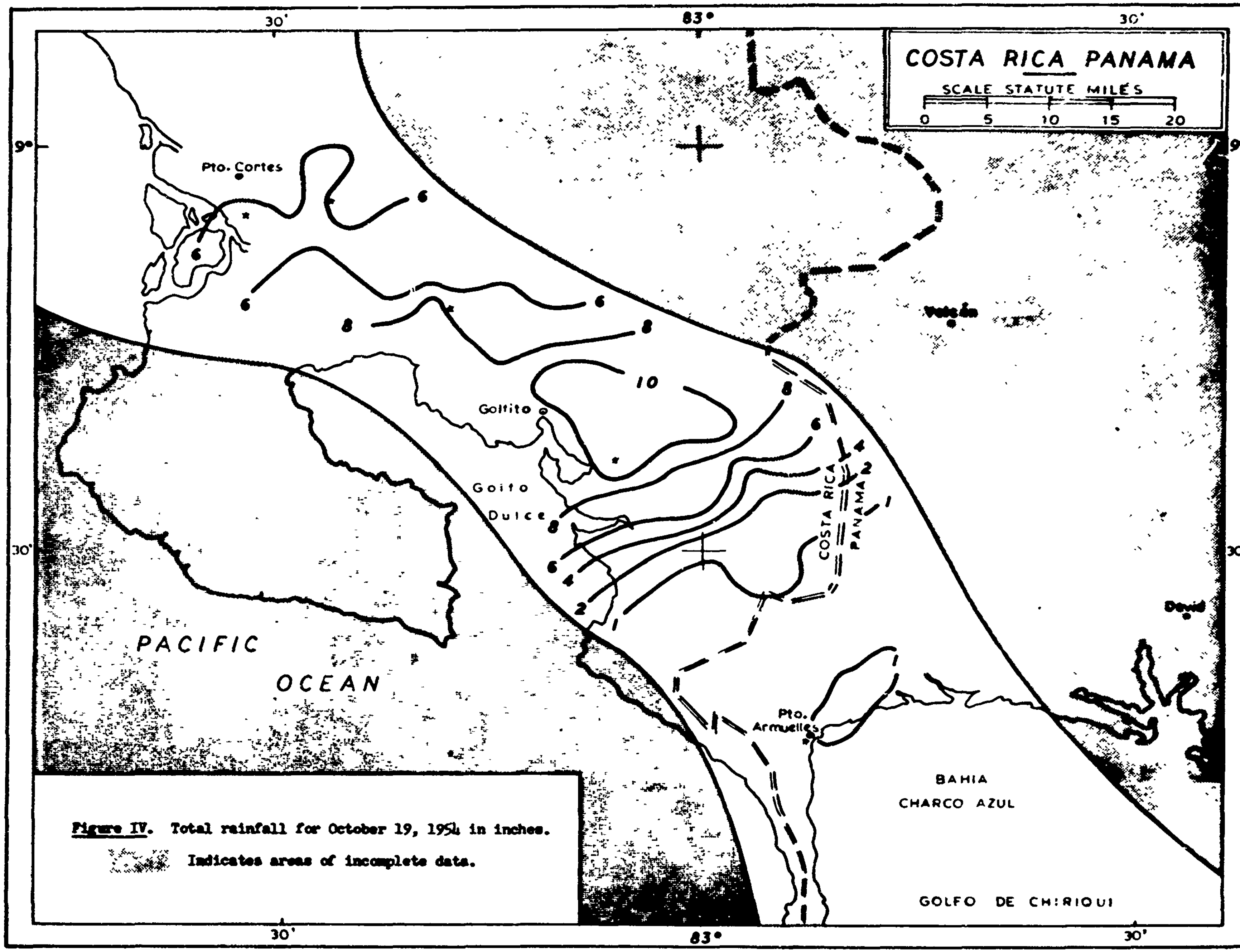


Figure IV. Total rainfall for October 19, 1954 in inches.

Indicates areas of incomplete data.

Statistical Analyses - J. F. Griffiths

(a) During the last Conference I described a method which could be used for the identification of the rainy season at the Central American stations. This method was applied to a number of stations, throughout the area, for which daily data covering a sufficient period were available. The results showed the feasibility of using such a technique but we are limited at present by not having daily data for a representative areal coverage of the complete region.

In Figure 1 the patterns for seven stations are shown in order to give some idea of the resulting distributions. The stations range over a distance of 500 miles. Manaca, the southermost site, shows the distinct dry season during March and April. Farm 41 (8°37'N, 82°57'W) in the Golfito-Puerto Armuelles area, indicates that the intensity of the dry season is lessening while at Seed Bed (8°45'N, 83°10'W) no dry period is identifiable. At Pozo Norte (8°[illegible]'N, 83°32'W), still on the west coast, a dry spell during February and March reappears. Moving to the eastern edge at Good Hope (10°04'N, 83°19'W) we observe a complete absence of a dry period. Siuna (13°40'N, 84°35'W) shows the pattern rather typical of the interior stations of Nicaragua, Honduras and El Salvador, namely a pronounced dry period with a relatively rapid change over from dry to wet. Nerone (15°37'N, 86°12'W) illustrates the variable pattern exhibited by the stations close to the coast where the local topography complicates the trajectories of the rain-bearing winds.

It is clear that real differences exist, but it is debatable whether the resulting information for mean patterns could not be obtained more rapidly from a typical classical analysis. However, the method is suitable for showing the year to year variations and it is hoped to use this to study the incidence of wet and dry years in large sections of Central and South America.

(b) In Report No. 7 we presented, among other items, an areal analysis of the standard deviation of annual and monthly parameters of rainfall. The comments I wish to make are concerned with Figures 22a, b, c, d (p. 119-122) of that Report, showing the isolines of annual standard deviation. As far as I am aware isolines of standard deviation of precipitation have not been areally presented before although standard deviations of temperature and the coefficient of variation of precipitation have been published. The map to which I refer is based on data for about 600 stations and should therefore have some reasonable degree of accuracy. However, as I drew this map I began to wonder how this could be interpreted. Incidentally, the remarks I make here apply also to the monthly maps, the annual being used for illustration.

The first deduction is that probability levels may be calculated by use of this map and the corresponding use of mean rainfall. This is definitely of use in practical problems. For instance, if we find

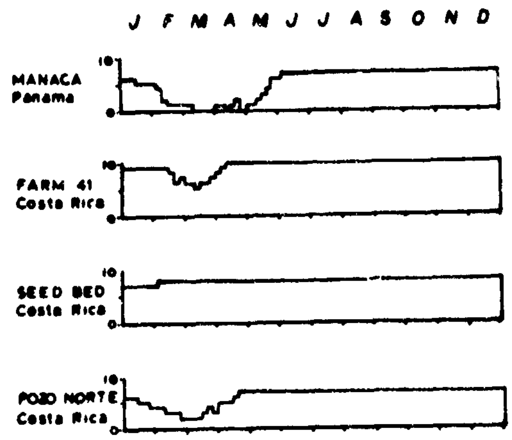


Figure 1. Frequency pattern showing the number of years on which each pentad was rainy, using the water budget technique.

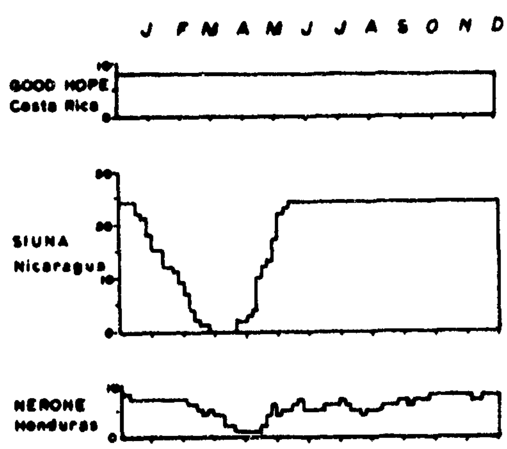


Figure 1. Frequency pattern showing the number of years on which each pentad was rainy, using the water budget technique.

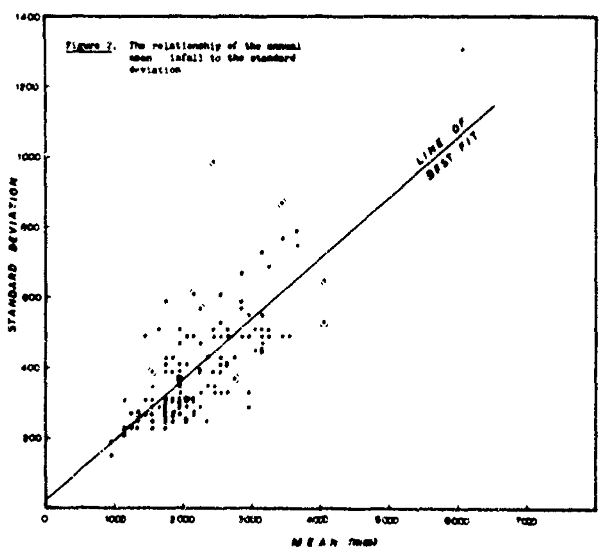
that for station A the mean value is $\bar{x}$ with a standard deviation of $\bar{\sigma}$ then the probability of receiving at least $\bar{y}$ can be calculated using

$$y = x \pm a\sigma$$

then solving for a and entering a table of the normal Gaussian function. For monthly values, $\bar{x}$ and $\bar{y}$ must be replaced by their square roots. The second deduction refers to the synoptic meaning of the map. It is agreed that, in general, the standard deviation increases with a higher average rainfall. However, deviations from such a relationship are many, varied and significant. Surely, in regions such as around San Jose where a 300 mm value is almost contiguous with a 700 mm value the direction of the prevailing winds must play a part. We need to understand more fully the macro-scale circulation if we are to interpret our meso-scale findings. A disconcerting aspect of the standard deviation maps is their apparent randomness of pattern, at least to the eyes of the author. We normally have a feeling, based on knowledge and experience, as to how isohyets are likely to be patterned in an area. Except for the rough relationship between $\bar{x}$ and $\bar{\sigma}$, mentioned above, the patterns of standard deviation often appear to pay no heed of such factors as topography and climatic areas.

(c) Earlier work has suggested a relationship between the values of mean and standard deviations at a station. In order to check this statistically the 129 stations with at least 20 years of data were selected. A linear regression line was fitted to these 129 pairs (Figure 2) with the following results -

(1) correlation coefficient = 0.746
(2) line of best fit $\sigma = 0.168\ \bar{x} + 30$ ($\bar{x}$, σ in mms)



The correlation coefficient, while being mathematically significant, is not sufficient to enable a practically useable calculation of σ from x to be made.

From the point of view of meso-scale analysis it is deviations from the line that identify the different regions. For instance, Graytown, Nicaragua, has a mean of 6018 mm and a standard deviation of 1315. The equation would give a value of 1102 for σ, a difference of twice the standard error of estimate. The standard error of estimate, 110 mm, is itself so large as to make practical use of this regression impossible.

(d) A plot of mean monthly rainfall amounts at a station generally shows a smooth curve of a composite trigonometric form. Because of this, and the well substantiated annual cycle, it is logical to suppose that this curve can be approximated by the method of harmonic analysis.

A preliminary investigation had indicated that the monthly values for San Salvador were well represented by the first three harmonics and the method was extended to cover 21 long-period stations in western El Salvador. The standard techniques of Fourier series analysis yielded the amplitudes and phase angles for these three harmonics given in Table I.

TABLE I

The amplitudes and phase angles of the first three harmonics of stations of W. El Salvador.

	First Harmonic		Second Harmonic		Third Harmonic	
Name of Station	Amplitude	Phase Angle	Amplitude	Phase Angle	Amplitude	Phase Angle
Acajutla	190	202	22	4	46	42
Ahuachaplan	209	198	32	44	43	40
Apopa	200	204	56	55	26	4
Ateos	164	196	25	35	31	62
Atiquizaya	193	196	22	51	47	44
Chalchuapa	190	202	41	12	36	83
Coatepeque	182	194	20	61	43	47
Comasagua	222	201	21	13	52	27
La Cabana	177	190	23	31	40	57
La Toma	206	195	30	46	43	55
Metapan	159	193	9	21	50	50
San Andres	130	194	22	60	29	44
San Jeronimo	154	193	11	73	47	43
San Salvador	184	195	15	47	43	40
Santa Ana	187	194	26	48	43	59
Santa Lucia	208	193	20	54	49	56
Santa Tecla	170	202	30	12	44	45
Sitio del Nino	165	195	21	46	31	50
Sonsonate	196	201	31	5	50	7
Tepecoya	216	196	36	10	42	51
Texis Junction	184	195	22	37	43	58

It is a debatable point as to whether, in general, in this chosen region there is much to be gained from the 2nd and 3rd harmonics since, as Table II shows, most of the variance is explained by the first harmonic.

TABLE II

The residual variances corresponding to the removal of the harmonics for stations in W. El Salvador.

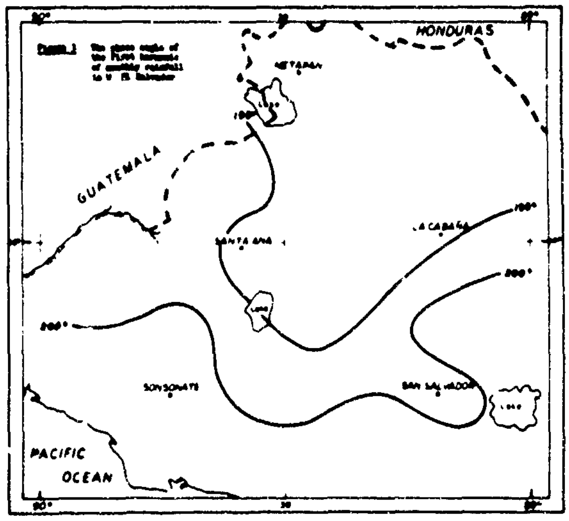
Name of Station	Residual variances (%) 1st Harmonic	1st & 2nd Harmonic	1st, 2nd & 3rd Harmonic
Acajutla	9	8	3
Ahuachaplan	9	7	3
Apopa	16	10	8
Ateos	9	7	4
Atiquizaya	9	8	3
Chalchuapa	11	7	3
Coatepeque	14	13	8
Comasagua	10	9	4
La Cabana	8	6	2
La Toma	8	6	3
Metapan	13	12	4
San Andres	10	8	6
San Jeronimo	13	12	4
San Salvador	7	6	1
Santa Ana	8	6	1
Santa Lucia	14	13	8
Santa Tecla	10	8	1
Sitio del Nino	7	6	3
Sonsonate	12	10	4
Tepecoya	10	8	4
Texis Junction	8	7	2

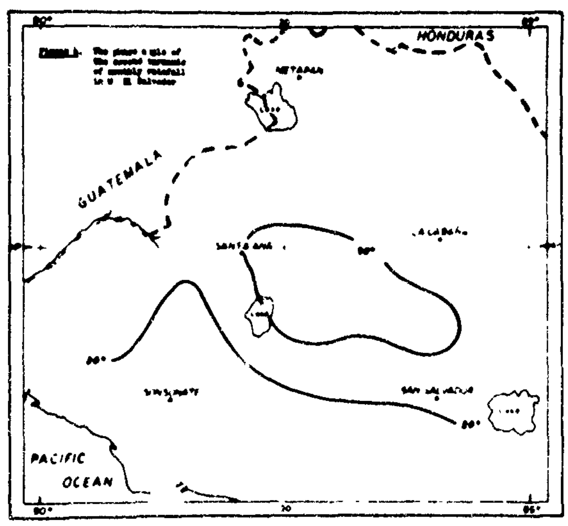
Owing to the dominance of the factor of the first harmonic a plot of the amplitude will approximate that of the mean annual rainfall. in pattern although not in magnitude. The factor of interest here is the phase angle. This will yield information as to the time at which rainfall occurrence reaches a maximum. Thus, the movement of a synoptic feature with which rainfall may be associated can be identified.

In Figure 3 the isolines of the first phase angle are shown. Remember that a phase angle of 180° corresponds to a maximum reached on July 16 and that a 1° change is approximately a 1 day shift. Hence a 200° line corresponds to a maximum on August 5.

In Figure 4 the second phase angle is indicated so that some idea of the magnitude of its variation can be obtained. a 20° line corresponds to about February 5.

It is likely that when this areal analysis is extended equatorwards the second and/or third harmonics will increase in significance.


HONDURAS
GUATEMALA
SANTA ANA
LA CABAÑA
SONSONATE
SAN SALVADOR
PACIFIC
OCEAN
100°
200°


HONDURAS
GUATEMALA
SANTA ANA
SAN SALVADOR
PACIFIC
OCEAN

DISCUSSION ON HENRY AND GRIFFITHS' PAPERS

WEICKMANN: Don't you have quite pronounced orography in this general area? Did you look at it to see if you couldn't explain the heavy rainfall from the orography?

HENRY: Yes, the area where this rainfall is, the section which I showed you, is mostly flat. However, just to the north, as you indicated, there are large mountains running through the backbone of Central America. As to what the rainfall might have been up in these mountains, I don't know. There is very limited data and I don't have any for 1954 from the mountains. Undoubtedly it would have an effect.

WEICKMANN: How many stations did you have for one of these maps?

HENRY: Approximately eighty to ninety.

QUESTION: Will you show us where the mountains are?

HENRY: Yes, the Volcań station is in the mountains. This is a mountain station right along this edge. Right about here is the edge of the mountains. There are a few hills in this section. This peninsula is mountainous, and just back of Golfito, the mountain goes up to 1500 feet, or so.

BARGMAN: I am quite sure everybody would want to ask whether you have, in fact, discovered any synoptic tie-up yourself, as yet?

GRIFFITHS: I usually hand this over to Professor Henry.

HENRY: No comment at this time.

GARSTANG: I think I might have asked this same question last year, and that is, it would seem to me that you have two problems. You are talking about representativeness. I think you might agree that if you had a rainfall station in the middle of the tropical ocean, this would be a representative station, but there would still be fluctuations on this station, depending upon synoptic changes. Do you not feel that the same criteria can be applied on your stations in Central America and, that is, if you can indeed classify these according to synoptic scale systems, the systems such as Professor Henry showed. You would indeed find quite good correlations between the various standard deviations as you have shown. You would find a good line of best fit, and the harmonic analysis that you showed is indeed representative of the frequency in the distribution of these systems. So, there is a direct tie-in here between the frequency of systems and the rainfall areas, that they produce. I wonder just to what extent you had thought of this?

GRIFFITHS: I only wish that I had a copy of last year's Proceedings, so that if your first premise is correct, I could give the same answer and not be held up for inconsistency. But I have checked one or two stations on their year to year variation and compared it with the harmonic analysis taken on their means over thirty years or more and again, of course, I get differences in numbers forecastable within their standard errors. This, I think, is the refinement to which I would like to turn when I feel that I have some knowledge of experience of working with these means. I am very glad to hear you say that you believe there is a real synoptic tie-up with this mathematics because that has bothered me a little. I just got lots of numbers.

GOLDMAN: I am having a lot of difficulty understanding just what is mesoscale. Isee a great record of data and I have a feeling that the term mesoscale now is being used just for its areal extent. Geographically it is smaller than some large pattern that you would have if you called it synoptic scale or macro scale. Is that correct?

GRIFFITHS: This is quite right. I think that these terms are used very loosely and I am sure we would all have our own definitions. As you have caught me up here on the platform, I suppose I will have to say my own. I feel that a mesoscale is something with a linear dimension of the order of a few tens of miles.

GOLDMAN: How about a time dimension?

GRIFFITHS: I'll agree that it should be there, but I have, of course, eliminated this to a degree by taking long period means in my initial investigations, but certainly there should be a time scale. The two must go together.

THE RELATION OF SUBTROPICAL HIGH MOVEMENTS TO RAINFALL IN THE AMERICAN TROPICS

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Texas A&M University

ABSTRACT

Eastward movement of the North Atlantic subtropical high pressure cell allows a southerly air flow from the Pacific Ocean to move over Central America and the Caribbean. Subsequent re-positioning of the subtropical high, with attendant shear zones and confluence of air streams from the Atlantic and Pacific, is related to the occurrence and distribution of rainfall.

1. INTRODUCTION

A study of occurrence and patterns of rainfall in Central America during the wet season suggests a correlation between the rainfall and the movement and intensity of the North Atlantic subtropical high pressure cell. Riehl (2) discussed a similar phenomenon at this conference last year. He showed that the rainfall of Southeast Asia was greatly affected by the intensification of the subtropical high and its movement westward.

The period of 6-9 June 1961 will be considered in this paper. The maps used for determining the synoptic situation and atmospheric flow were those routinely prepared by the weather station at Albrook Air Force Base in the Canal Zone. Position and movement of the subtropical high were determined from the nothern hemisphere surface charts prepared by the U. S. Weather Bureau. The rainfall information was extracted from the daily rainfall records that have been collected from the various countries in Central America.

Figures 1, 2 and 3 depict the streamline flow for the 850, 700 and 250 mb levels respectively at 1200 GMT 7 June 1961. Figures 4, 5 and 6 show the streamline flow for the same levels at 0000 GMT 10 June 1961. Associated rainfall areas are shown on the 850 mb charts also. These do not pretend to be detailed rainfall analyses but give a general view of the rainfall patterns. Of the total stations, only a representative sample within a given area was used because of the problem of illustration on the map. Likewise, all of the rainfall reports within a depicted area were not of the indicated magnitude; rather, the depicted magnitudes are typical of the reports within the indicated areas. It should be remembered also that rainfall records are available for limited areas only so that a complete picture of the rainfall distribution is not possible. Table 1 is a list of the rainfall amounts that fell at selected stations on the days under consideration.

2. POSITION OF THE SUBTROPICAL HIGH

The northern hemisphere surface charts indicated that there was an eastward movement of the North Atlantic subtropical high during 1-5 June 1961. The center of the high pressure shifted from 32°N 45°W to 38°N 28°W and the central pressure increased from 1029 mb to 1037 mb. The pressure gradient over the Caribbean decreased on 5-6 June, even though the position of the pressure center returned westward to 37°N 36°W by 0000 GMT 7 June. This was followed by a decrease in the central pressure to 1030 mb and a brief eastward shift on 8 June preceded gradual westward movement during the following week. It should be noted that the high's movements and central pressure changes were related to movements of high and low pressure systems from the United States into the Atlantic Ocean.

Coincident with the eastward shift of the subtropical high cell and the weakened pressure gradient over the Caribbean was the penetration of southerly flow over Panama and Costa Rica. This southerly flow was rather weak on the 850 mb chart of 6 June and did not appear on any higher level charts, but it had strengthened by 7 June, and was evident at 700 mb also. The streamlines on the 850 mb chart (Fig. 1) indicate that a shear line was present between the southerly flow and the easterly flow from the subtropical high. This, of course, was an area of high vorticity which contributed to general storm development.

3. ASSOCIATED RAINFALL DISTRIBUTION

On 7 June 1961, the areas of rainfall amounts greater than one inch were confined generally to the Pacific side of the countries, extending from southwestern Panama to southeastern El Salvador. Scattered amounts of less than one inch occurred east of the shear line (Fig. 1). The 700 mb chart (Fig. 2) shows a col and light convergent winds over Central America; divergence existed at 250 mb (Fig. 3) over the same area, with a trough extending from northwest Florida to Belice (British Honduras).

The importance of convection and orographic lifting is evident from the rainfall distribution. The larger amounts occurred predominantly in areas of orographic uplift, although there were some heavy rainfalls in the coastal plains where the warm, moist air moved onshore and immediately was subjected to convective processes. It is interesting to note that most of the rain falling in El Salvador was in the southeastern part of the country where air flow was directly inland, while very little rain was reported in the northwestern section where there was less shear and the flow was more parallel to the coast.

Mountain ranges are found in all of the countries, dividing them into Caribbean and Pacific parts and influencing the air flow. Thus, the downslope motion of air as it moved to the Caribbean side suppresse heavy rainfall there. The rains which did occur resulted from orographic uplift of the easterly flow from the Caribbean, and the 250 mb trough was a possible factor in the precipitation over northern Honduras and eastern Guatemala.

The rainfall on 6 June was somewhat similar to 7 June in distribution but smaller in amount. The greatest quantities fell along the Costa Rica-Panama border where the southerly flow initially arrived over the land mass and was subjected to immediate orographic effects.

The eastward shift of the subtropical high on 8 June allowed further penetration of the southerly flow over the Caribbean and also a low over Florida began developing. At 850 mb, circulation into a convergent point located on the north side of the isthmus was associated with increased rainfall on the Caribbean side of Costa Rica and Panama.

The return towards the west of the subtropical high on 9 June resulted in stronger flow over the western Caribbean. By 0000 GMT 10 June, the streamline flow was as shown in Figures 4, 5 and 6. Shear lines existed on the 850 mb chart as indicated, with convergent points over Florida and Guatemala-Honduras. The 700 mb flow was rather weak, but apparently the cyclonic circulation over Florida was the dominant feature. The 250 mb trough was now orientated north-south from Florida to the Canal Zone. The rainfall was generally more widespread and heavier than on any of the previous days. Amounts greater than one inch were common along the Pacific coast, with more than two inches reported at a number of locations in El Salvador, Nicaragua and Costa Rica.

4. CONCLUDING REMARKS

There appears to be some correlation between the movement of the North Atlantic high and rainfall distribution in the American tropics. It is possible that there might be interaction between this high and the southern hemisphere Pacific subtropical high, similar to that found in Africa by Johnson and Mörth (1), but this could not be examined due to a lack of map coverage in the South Pacific.

An eastward shift of the Bermuda high and weakened pressure gradient in the Caribbean permit southerly flow from the Pacific Ocean to be established over Central America. Movement back to the west of the Bermuda high strengthens the easterly flow in the Caribbean and the resulting shear zones and areas of convergent flow are the most favored regions for rainfall occurrence. It must be remembered, however, that local effects such as topography and convection are of primary importance. The Pacific side of the countries receives a large percentage of the rain resulting from the southerly flow, and those stations on the windward side of an orographic barrier (or in a basin into which the air flow is channeled) normally record the largest quantities. Thus, rainfall occurrence is a product of both macroscale circulation features and local effects; the macroscale circulation may either inhance or suppress the local effects and the mesoscale activity.

When the easterly flow is sufficiently strong it will replace the southerly flow over the land mass, which happens during the dry season. This suggests that the so-called intertropical convergence zone in this part of the world is a product of the interaction of the northern and southern hemisphere subtropical highs, and the ITCZ appears, disappears, intensifies, or weakens in response to the degree of interaction between the highs.

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TABLE 1.

DAILY RAINFALL* AT SELECTED STATIONS IN CENTRAL AMERICA, 6-9 JUNE 1961

PACIFIC SIDE					CARIBBEAN SIDE				
Date:	6	7	8	9		6	7	8	9
C A N A L Z O N E and P A N A M A									
Balboa Hts	1.2		.4	.9	Cristobal	1.2	.9	.2	.2
Armuelles	1.7	.05	.5	.3	Changuinola	.6	.1	.2	.3
Farm Blanco	.3	2.3	.1	.1	Farm Chase		.5		.6
" Maria	.3	3.1	.1	.1	" Margarita	.1	.4		.7
" Manaca	1.0	.6	2.5	.2	" Nievecita		1.0		1.0
C O S T A R I C A									
Farm 41	1.9	2.0	.5	3.0	Limon	.2	.3	.7	.2
Coto (Km.18)	1.0	1.3	.0	2.3	Finca 16	.1	.07	.4	.4
Golfito	2.6	.1	4.8	.4	Pto. Viejo	.4	.2	.8	1.8
Puntarenas	.5	.2	.8	1.2	Los Llanos	.04		.1	2.7
San Jose	.4	1.2	.5	2.1	Turrialba	.2	.3	1.2	1.2
Villa Mills	.3	1.1	.2	1.8	Sasso-Pirie	.3	.4	1.5	1.7
N I C A R A G U A									
Managua				2.0	Bluefields	.6	3.2	.05	1.6
Jobo Dulce	.7	.7	.6	4.7	Siuna		.2		2.5
Farm Polvon	1.2	.2	1.5	2.4	Bonanza	.4	.1	.3	1.3
Corinto	1.3	.1	.5	1.3					
El Paraiso	.5		.8	1.5					
E L S A L V A D O R					**H O N D U R A S**				
Acajutla		.04	1.1	4.1	La Ceiba	.6			
Sonsonate		.1	.3	2.0	Tela	.04			
Olocuita	.3			3.2	San Pedro Sula	.3			
Usulutan				1.4	Tegucigalpa		.2		1.5
La Union		1.1	1.5	3.2	Yoro	.05	.06	.5	.9
Sto. de Maria		1.1	1.9	.3	Chumbagua	1.7	1.3		
San Vicente		1.5			Santa Rosa de Copan	.6	1.2		.05
San Salvador	.04			.3					
Chalchuapa	.6	.4		1.1	Campamento Las Moras	.02	.4	.2	.01
Chalatenango		.8	2.01						
G U A T A M A L A									
Guate. City	.4		.2		Barrios	.1			
Chocola		.5	.9	.6	Farm Las Brisas	.2			
					Bananera	.3	.4		.4
					Farm Aztec		.1	.1	.3

*Rainfall accumulated between 1300 GMT of the date indicated and the following day. (in inches)

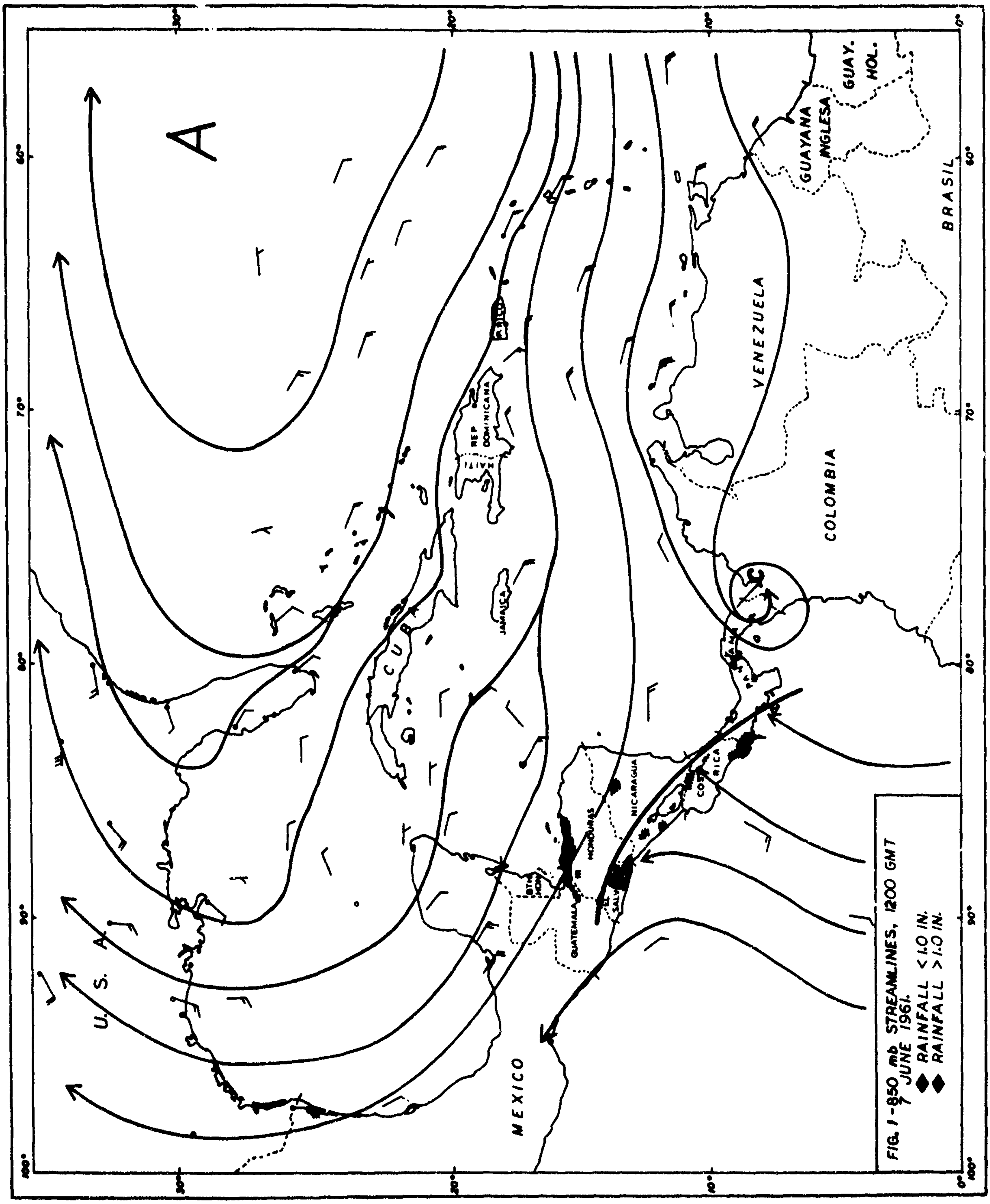

FIG. 1 - 850 mb STREAMLINES, 1200 GMT 7 JUNE 1961.

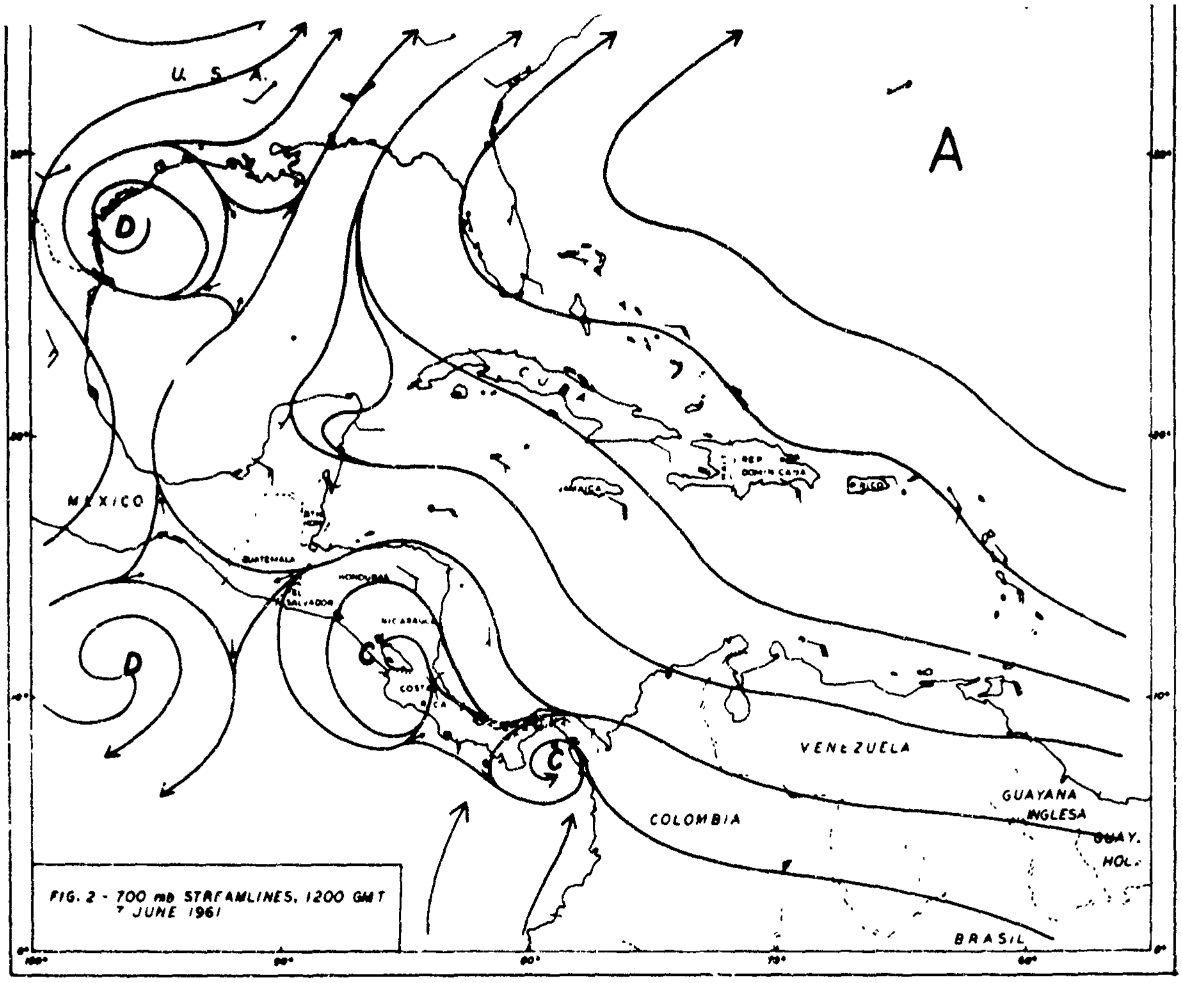

FIG. 2 - 700 mb STREAMLINES, 1200 GMT 7 JUNE 1961

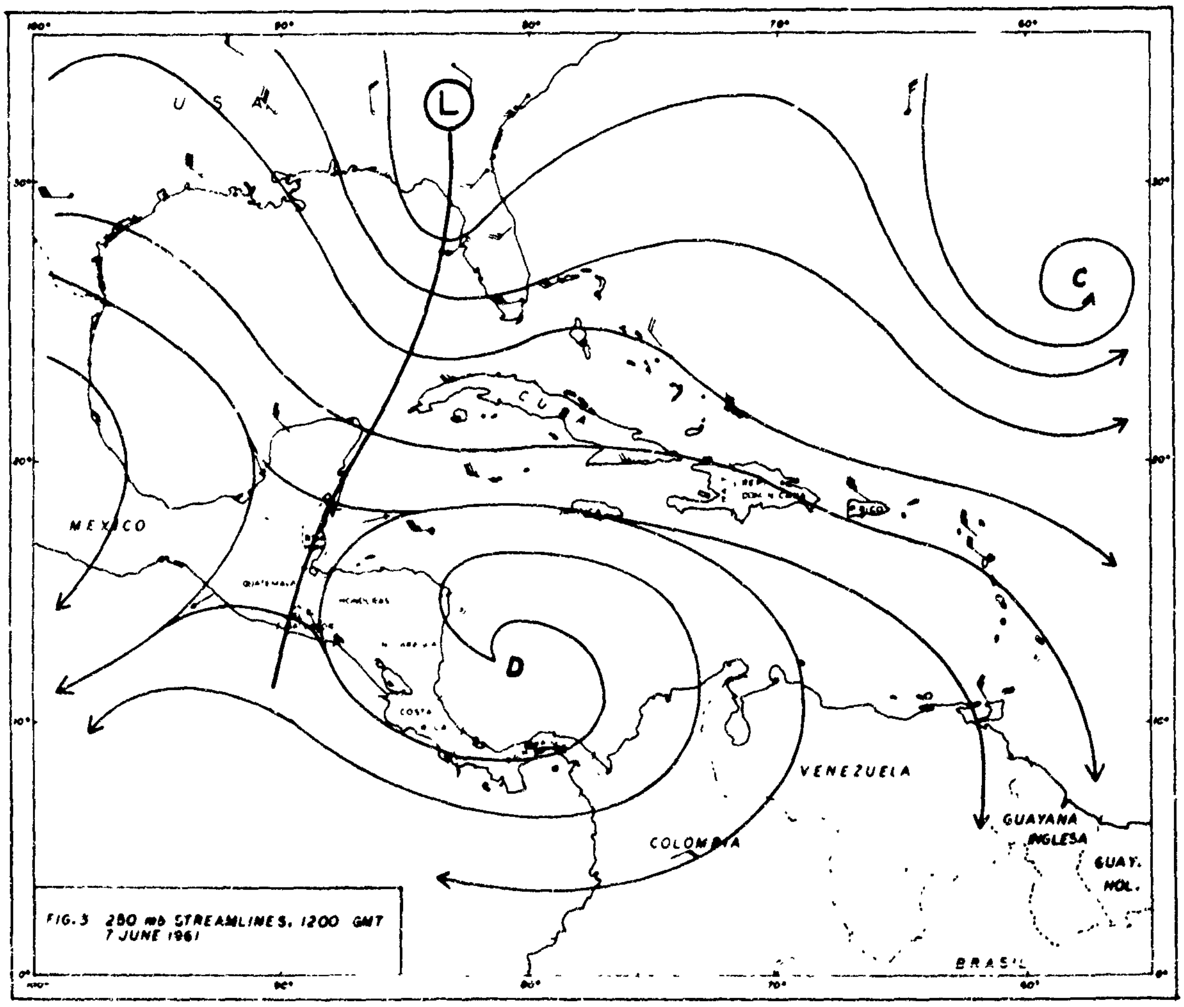

FIG. 3 250 mb STREAMLINES, 1200 GMT 7 JUNE 1961

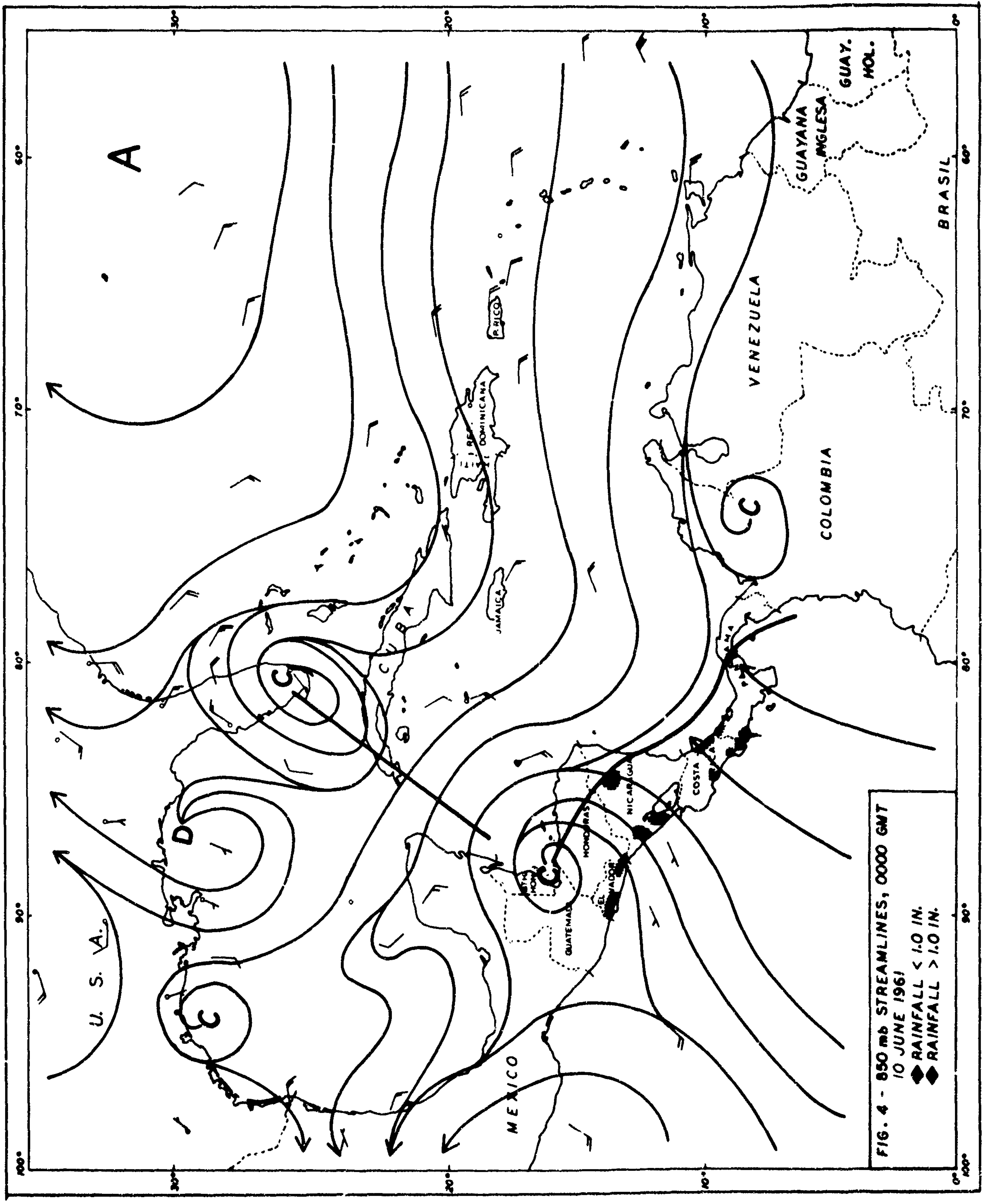

FIG. 4 - 850 mb STREAMLINES, 0000 GMT 10 JUNE 1961

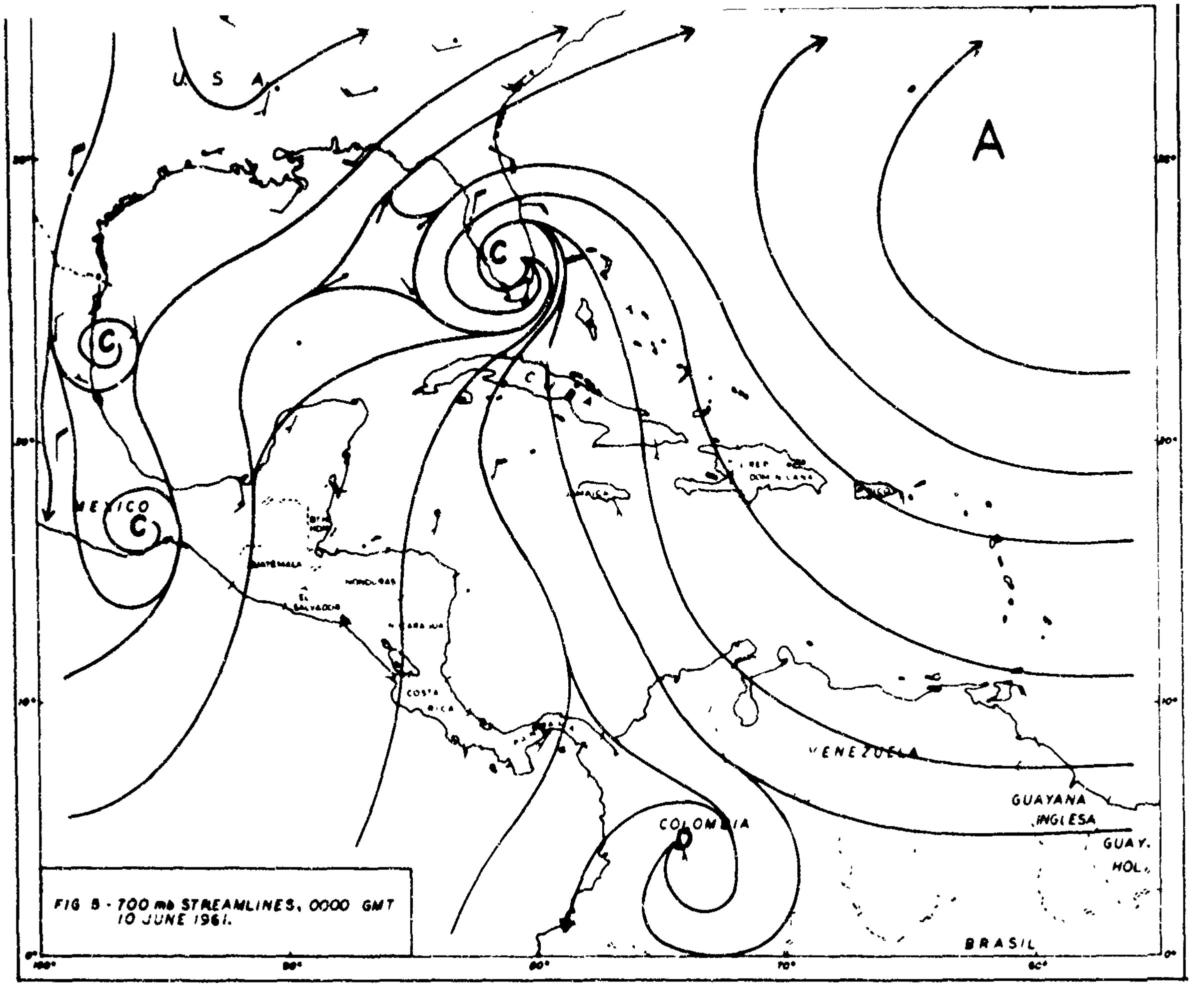

FIG 5 - 700 mb STREAMLINES, 0000 GMT 10 JUNE 1961.

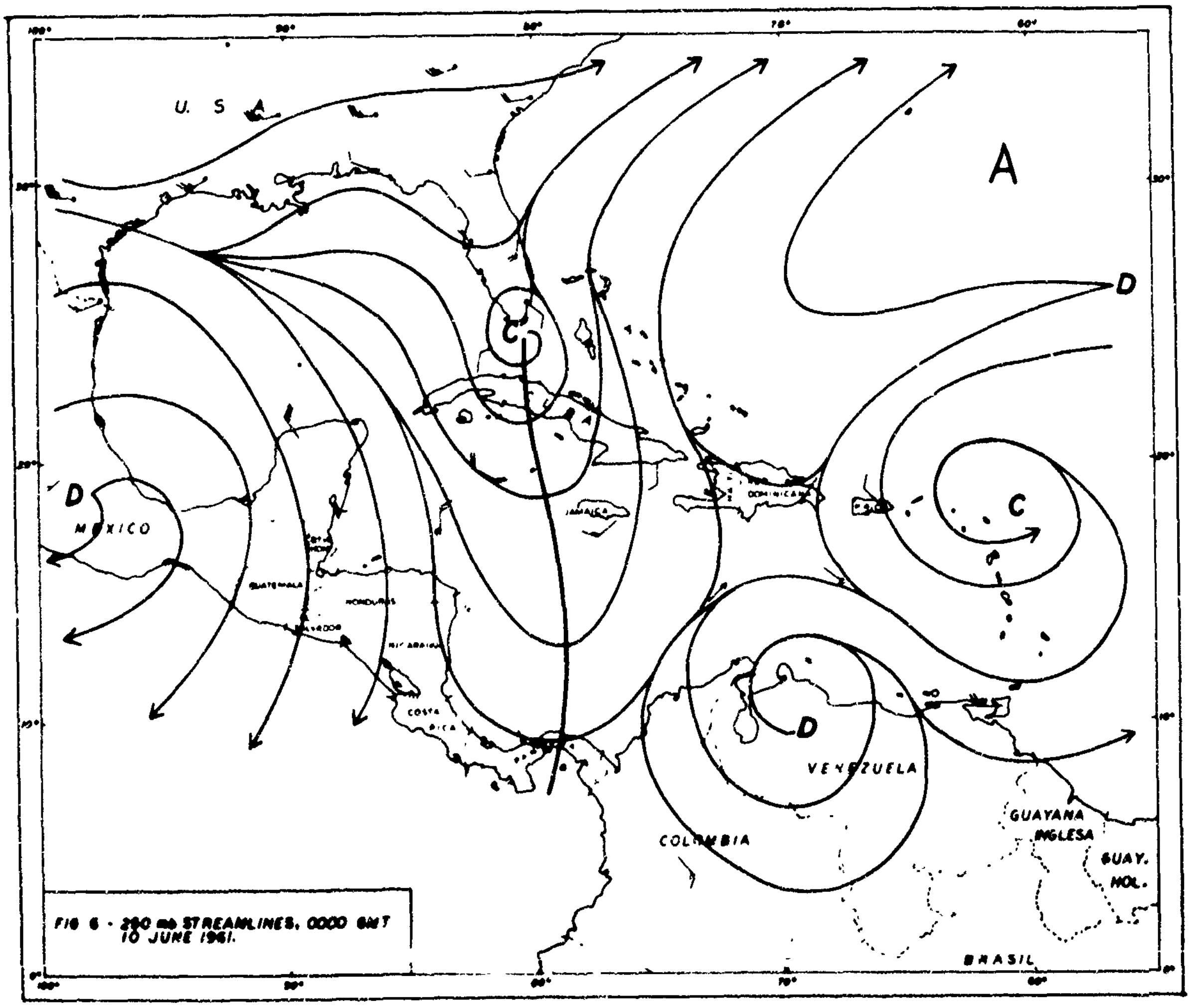

FIG 6 - 200 mb STREAMLINES, 0000 GMT 10 JUNE 1961.

DISCUSSION ON COBB'S PAPER

HUBERT: The suggestion that you made toward the end that it might have something to do with the interaction of the South Pacific high, if you look at some of the work of Col. Saddler, he shows that the climatology of this region is rather more of a monsoon southwesterly flow and that this might have only a very indirect, if any, connection with the southern Pacific high. It would seem that it might be variations in strength of the southwest monsoon that extends perhaps out to 130 or 140 degrees west even, and the south Pacific high is very removed from that. I was just wondering if there was any possibility of your examining the effect of the southwest monsoon rather than trying to hook it up to the even more distant south Pacific high?

COBB: This is a good suggestion. I haven't seen Col. Saddler's work and this problem of the high in the Pacific is hard to solve since we have no data. This was an idea that occurred to me that it might be interaction between the two, because after reading some of the work of Johnson and Morth, I believe in Africa, they found that interactions between highs across the equator were associated with rainfall patterns, a strengthening of the rainfall patterns when there was a drift, I believe. So, I looked at it from this aspect, but your suggestion is good.

BARGMAN: Well, if I can comment there. You're thinking of what we call a duct in East Africa, and certainly I would subscribe to your opinion that there is possible interaction between the high pressure cells north and south of the equatorial trough.

GARSTANG: I wonder to what extent you considered laying the emphasis on the mid-Atlantic trough, rather than the position of the high, although both features are obviously related to each other? It seems that many of the vortices that you showed on your maps were associated with this trough and what perhaps one might interpret, not as shear lines, but as asymptotes in that wind field, in fact, they were very much like spiral bands in some cases that you were showing. Those spiral bands moving around these vortices over the mainland would perhaps also explain just as well the occurrence of rainfall as you have shown it to occur, as well as the fact that it is on-shore flow over the orographic ridges.

COBB: My feeling so far has been that this trough that we have had in general is influenced in its movements and its intensity by the intensity and movements of the high pressure system, so that they are all interacting on each other. The low moves and intensifies or weakens in response to what happens in your large-scale subtropical movements and intensity.

LESSMAN: I think we have to be very cautious to speak about orographic effects. We often observe that the highest intensity of showers is not in the mountains, but in the plains. You showed some examples from El Salvador. There was at least one station in the mountains that hardly had any rainfall, but in the coastal plains there was high rainfall. Then also I think that in Eastern El Salvador there is a very important mesoscale effect by the Gulf of Fonseca.

It seems that this is the origin of very strong thunderstorms developing in this area. Then, I like to underline that the interaction of the easterlies and the advection from the Pacific Ocean produces the most intensive showers on the Pacific side. Showers up to 180 millimeters were recorded which fell within some 4 hours.

COBB: In response to your comment about the orographic effects, the two seasons that I was in southwestern Panama this was also evident, but still that which cccurred in the plains was in response quite often to that which began on the so-called mountain slopes, or the foothills. Quite often the sea breeze, or either an easterly wave that would begin on the foothills, would then begin what we called or term a "walk out" back toward the ocean and the much heavier amounts would fall over the plains rather than over the mountains. When I said orographic effects, I didn't mean to imply that the heavier amounts would fall at the mountain stations, but I did imply that the mountains did affect the amounts which were falling in the plains very much. There is a line of hills in this area. There is another hill about 2700 feet in this point. The other foothills are in this point, so that you would think that the heaviest rainfall should fall right in this area. But, at early afternoon or perhaps even at 10 o'clock in the morning, the clouds will begin building here and they will begin building here. During the afternoon, they would invariably form a line in between the so-called plains where the elevation was, no more than 150 feet, yet was much heavier than what had occurred here. All your mountains were up here. The clouds began to form in here. The winds may be from this direction, but as rainfall started, the colder temperature couldn't move up slopes, so it flowed down slope and in greater storms would form in this area and continue down. Thus, the heaviest rains would occur in this area along here, in the plains, but yet it was affected by orographic effects and this is what I implied when I said orographic effects. I agree we must have caution in the way we throw terms around.

KOTESWARAM: I should like to add two comments with respect to similar phenomena from India. One relates to this orographic effects. There also we find that quite often the heavier rainfall occurs on the plains rather than on the hills nearby. We generally felt that all the monsoon rains emptied themselves on the Western Ghats, a chain of mountains in the west of India, but there are cases in which the very heavy rain occurs in the plain station to the west of it, and the rainfall sort of diminishes as it goes inland right up on the mountain slopes. The second one would be that with reference to this monsoon phenomena on the coast in the area in which you are interested, I shall be very much interested, actually, to think in terms of the monsoon over there because we feel that here is an area which is almost similar to the area in India except, of course, for the fact that there are no Himalayas there. We have got the contrast with the land and the sea, the heated land and the cold sea, where you've got the monsoon over there. Similarly you've got a similar monsoon on a much smaller degree than on the west coast of Africa, the old Gold Coast, the present Ghana coast. Similarly there is a monsoon phenomena of the type which has been pointed out by Mr. Hubert, who has worked in investigating in this part of the world, too.

RAINFALL MESO-SCALE PATTERNS IN EL SALVADOR, CENTRAL AMERICA

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ABSTRACT

Rainfall meso-scale patterns are shown by authomatic rainfall records and raingauge data. Productiviness, extention and other features of showers are discussed. The problematic behaviour concerning its motion and orographic effects on tropical showers are outlined.

1. INTRODUCTION

By radar we see the rainfalls or showers and even the individual convective cells which form the shower. Crow and Cobb (1) showed this for Southwest Panama and gave interesting details of rain within the tropics in a mesoscale. Griffiths and Henry (2) gave also examples of meso-scale analysis of rainfall from the same area. They concluded that one or more meso-scale shower systems produce more than 25 mm of rainfall. Each meso-scale system has a diameter of about 5 miles or 8 km and is surrounded by weaker rains up to about 6 miles or 9 km from the center. Each meso-scale system consists of a cluster of convective cells for which Crew and Cobb found an average lifetime of approximately 24 minutes. Cells with a longer lifetime have convective clouds which grow up above the "freezing zone" (in this area more than 20.000 feet or 6.000 meters with less than -10°C).

2. MESO-SCALE SHOWER SYSTEMS IN EL SALVADOR

Let me demonstrate an attempt made to analyze meso-scale shower systems by the data of a dense network of raingauge and rainfall recording stations, which existed during the year of 1956 around San Salvador, which is the capital of El Salvador (13°43' N, 89°12' W, 700 m), including inner valleys with 500 to 800 m elevation and mountains up to nearly 2.000 m above sea level. Over an area of about 800 square kilometers 28 raingauges and 6 rainfall recorders; five of the recorders had distance from each other between 2 and 11 km, and only one was 24 km away.

2.1 Synoptic situations

I like to demonstrate three examples, all of June 1956 (Figs.1 to 6):

June 4 : an easterly (instability) wave (3,4) passed between 17 and 20 hours local t me;
June 8/9 : an easterly (instability) wave was combined with a trough (arrived about 18 hours local time) and an instability zone coming from the SW;
June 16/17 : similar to the situation of June 8/9, an instability zone from the SW arrived about 03 hs. local time,after having been observed during about ten hours thunderstorms and lightnings.

The rainfall recorder data made it possible to split the daily rainfall amounts into single rainfalls, and these are the results.

2.2 Shower productiviness and extention

High daily rainfall amounts can be produced by a single strong shower; i.e. June 8/9: 98 mm in Santa Tecla and 85 mm in Ilopango Airport; each of these showers which ocurred nearly at the same time had a diameter of about 10 to 15 km (considering the 50 mm isohyete) and were separated one from the other by a less productive zone (minimum 35 mm!) of about 7 km width (fig. No. 4). The authomatic records show the single showers as meso-scale systems, also those which produced only small amounts. An analysis of two separated showers of June 4 (Figs. No. 1, 2 and 3) lead to a size of 4 to 12 km width and 21 to 18 km length, respectively (following the 25 mm isohyete proposed by Griffiths and Henry). Another single shower (not shown here) which fell on 15 March 1958 during the dry season and produced up to 113 mm within 2 hours or less had a width of about 20 km and a length of 70 km at least.

2.3 Single cells

In some cases it was possible to distinguish single cells, -probably identical to those seen by Crew and Cobb by radar,- which compose the shower systems; they were clearly separated in the authomatic records from the main showers and produced less than one milimeter. Following the results of Crow and Cobb, we may estimate from our records that the small rainfall producing cells primarily had a short lifetime of approximately 10 minutes. Probably they all were produced by simple water cumuli without reaching the freezing zone. Another class with a lifetime between 32 and 44 minutes producing from 4 to 16 mm, probably originated from an iced cumulonimbus with a height of more than 20.000 feet.

2.4 Shower character

Most of the rain of the heavy showers (88% to 96%) of long duration (i.e. June 8/9, about 6 hours) was produced during the first two hours; then they transformed into a weak rain without shower character, we named "post rain of trough" (3).

2.5 Horizontal motion of showers

Generally there was no clear horizontal motion of showers. In some cases the shower system began to rain near the San Salvador Volcano (in the figures indicated by the 1500 m isohypse, the crater and the highest point with 1958 m above sea level) first and somewhat later more eastward over the free plain, producing over the latter on June 8/9 (Fig. No. 4) the same or even larger quantities, and on June 4 (Fig. No. 3) smaller ones or even nothing. On June 17 (Fig. No. 6) and 25 (not shown here) the showers fell first over the plain and later near the mountains. On June 16/17, a heavy shower fell in San Andrés (in Fig. No. 5 on the left side marked with 53 mm) between 23:35 and 03:42 hs. local time, producing 36 mm, and another one from 05:30 to 07:34 hs. with 18 mm; those two showers fell before, during and after the heavy shower shown on Figs. No. 5 and 6 over the Capital valley in a distance of only 20 km to the WNW of the Capital. These facts really make it difficult to recover a horizontal motion of showers. The author observed and heard several times standing on a high vantage point some 300 m above the valley of the Capital, that showers crossed the valley, not more than several hundred meters from his observation point, which received not a single drop of rain! - Only in some cases it was possible to note a horizontal motion. The shower shown in Fig. No. 6 over the Capital valley, possibly moved from SE to NW with about 22 km/h (see the remarks made some sentences before). But, if we presume a E - W motion, the speed between Ilopango Airport (rainfall begin 02:28 hs.) and the Capital (02:39 hs.), -distance 10 km, - was about 50 km/h. In another case, on June 25, we estimated a velocity of 40 to 50 km/h.

Although there were no upper-wind data available, the synoptic situation and surface wind records in Ilopango and San Salvador indicate that probably there were no mid-tropospheric flows (500 mb) as strong as about 40 to 50 km/h. Our opinion is that the relatively slow current which drove the shower systems was covered up by the faster advected instability parameter (easterly wave) (3.4), causing an apparently unsystematic vertical upward motion like bubbles, producing a disorderly rainfall activity over larger areas; Crow and Cobb found similar features by radar in SW Panama.

2.6 Orographic effects

Orographic effect, too could not be well recognized. The Figs. No. 2, 3, 4 and 6 show that the volcano received less rain than the valley of the Capital. There seems to be a certain luff (to the E and SE of the volcano) and lee efect (to the SW and W), but probably it is accidently in the cases here presented. The yearly average rainfall for the top of the volcano is about 200 to 300 mm higher than that for the Capital valley (2050 and 1750 to 1900 mm, respectively). The difference is samll considering the difference of height of 1300 meters (6).

It shall be mentioned here that in general the sea-breeze does not promote the shower activity, except when a large-scale southern flow is superimposed. Between the Capital and the Pacific coast (distance 22 km to the South), lies a mountain range about 1000 m above sea level (see Figs. 1 - 6). The sea-breeze clouds lie on the ridge, but during ten years the author never observed any rainfall from them; only on the ridge within the clouds sometimes a fine drizzle was felt. When the sea-breeze in the afternoon (between 14 and 15 hs. local time) invades the Capital valley and surrounding plains, it brakes the convective cloud development over this area and sometimes dissolves it (5). Probably the coastal mountain range is not high enough to support the hot and humid sea-breeze air mass to rise up to the freezing zone in about 6000 m above sea level, which would be necessary to produce regular showers, like in Western

Guatemala (volcanic mountain range between 2500 and nearly 4000 m above sea level), in SW Panama (1), and possibly some 40 km ESE from San Salvador in the volcano massive San Vicente (nearly 2200 m above sea level).

3. CONCLUSIONS

3.1 It was tried to show by some examples that in tropical areas a relative dense rainfall recorder (with a minimum resolution of 2 cm for each hour and 1 cm for each millimeter of rain) and raingauge network is very useful to reveal more details of rainfall meso-scale patterns. For developing countries is has the adventages of relatively low costs and to be easily operated and maintained by unskilled observers. A frequent inspection of the stations seems to be indispensable.

3.2 In the tropics horizontal motion of showers and orographic effects on them ara not yet well understood. A combination of radar and authomatic rainfall recorders will be able to reveal their behaviours.

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RAINFALL MESO-SCALE PATTERNS IN EL SALVADOR, CENTRAL AMERICA

Helmut Lessmann

DESCRIPTION OF GRAPHICS

Fig. No. 1:
Daily rainfall amount, June 4 (07 hs.) to 5 (07 hs. local time), 1956 in San Salvador and surroundings (in "millimeters"), produced by two showers: 17:20 - 19:10 hs. and 20:10 - 00:55 hs. local time (only in San Andrés fell a third shower (18:50-20:45 hs.) producing 8 mm): isohypses in "meters" above sea level.

Fig. No. 2:
Rainfall amount produced on June 4, 1956, in San Salvador and surroundings (in "millimeters") by the shower in the afternoon (17:20 - 19:10 hs. local time).

Fig. No. 3:
Rainfall amount produced on June 4/5, 1956 in San Salvador and surroundings (in "millimeters") by the shower at night (20:10 - 00:55 hs. local time).

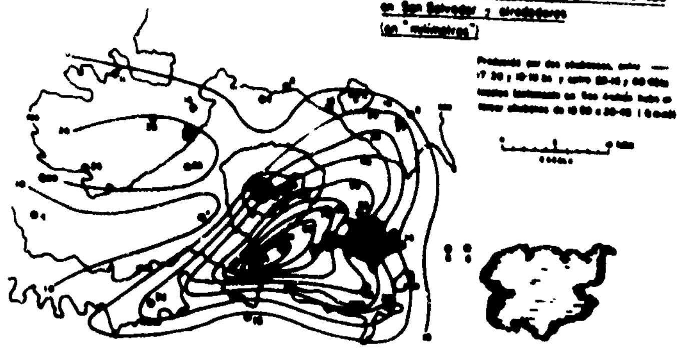
Fig. No. 4:
Daily rainfall amount, June 8 (07 hs.) to 9 (07 hs. local time), 1956, - note, in the graphic the dates June 7 and 8 are erroneous, in San Salvador and surroundings (in "millimeters"). It was produced by a single shower of long duration, between 17:23 and 00:49 hs. local time; only in Apopa (with 42 mm, 10 km to the North of the Capital) the total amount was produced by two showers, one in the afternoon (4 mm) and the other was part of the main one at night (38 mm).

Fig. No. 5:
Daily rainfall amount, June 16 (07 hs.) to 17 (07 hs. local time), 1956, in San Salvador and surroundings (in "millimeters").

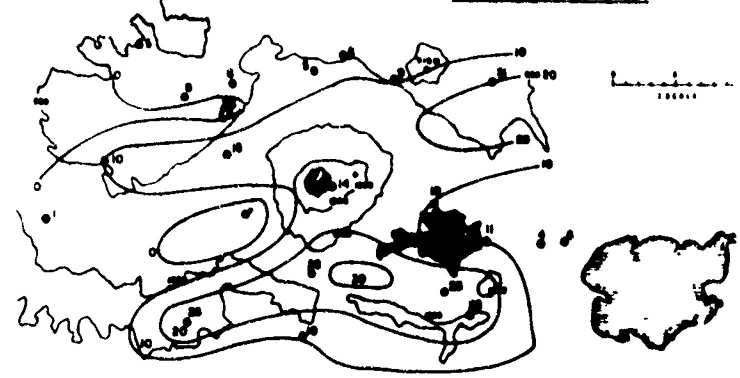
Fig. No. 6:
Rainfall amount produced by a heavy shower of long duration, on June 17, 1956, between 02:28 and 06:12 hs. local time (in "millimeters"). The time of rainfall beginning is indicated at each rainfall recorder station. The arrow marks the supposed direction of the shower motion: the speed was estimated to 16 km in 44 minutes, equal to 22 km/h.

-.-.-.-.-.-.-.-.-.-.-

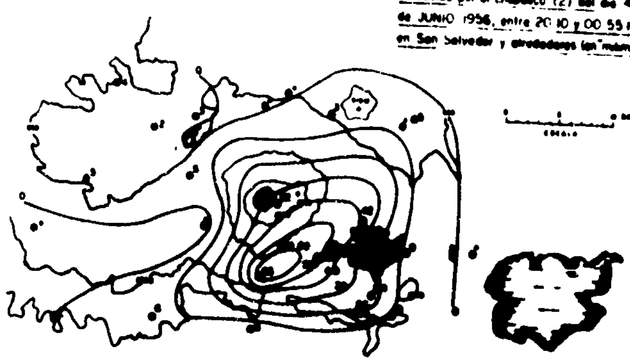
No. 1 Cantidad Diaria de LLUVIA del dia 4 (07 hs) al dia 5 (07 hs) de JUNIO 1956 en San Salvador y alrededores (en "milimetros")



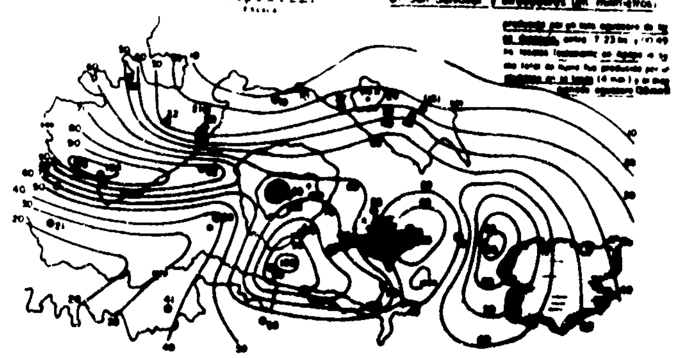
No. 2 Cantidad de Agua LLUVIA producida por el chubasco (1) del 4 de J entre 17 20 y 19 10 hs locales en San Sa alrededores (en "milimetros")



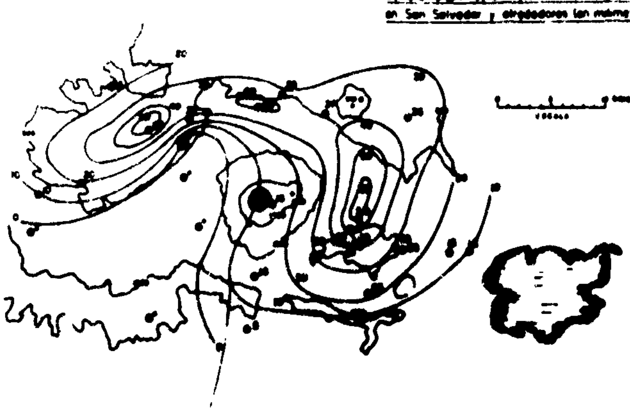
No. 3 Cantidad de agua LLUVIA Producido por el chubasco (2) del dia 4/5 de JUNIO 1956, entre 20 10 y 00 55 hs locales en San Salvador y alrededores (en "milimetros")



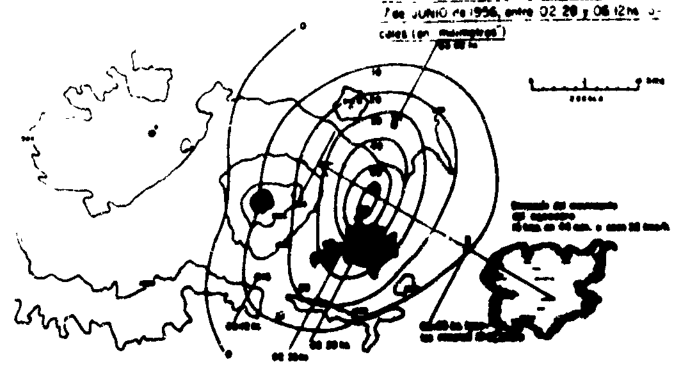
No. 4 Cantidad Diaria de LLUVIA del dia 7 (07 hs) al dia 8 (07 hs) de JUNIO 1956 en San Salvador y alrededores (en "milimetros")



No. 5 Cantidad Diaria de LLUVIA del dia 16 (07 hs) al 17 (07 hs) de JUNIO 1956 en San Salvador y alrededores (en milimetros)



No. 6 Cantidad de Agua LLUVIA producida por el Aguacero de larga duracion el dia 7 de JUNIO de 1956, entre 02 20 y 06 12 hs locales (en "milimetros")



DISCUSSION ON LESSMAN'S PAPER

WEICKMANN: You had rain of approximately 100 millimeter intensity. What was the duration of the shower?

LESSMAN: This lasted some four hours, from 2000 hours to after midnight.

WEICKMANN: So what you called a shower would more likely be very heavy extended rain.

LESSMAN: It's a convective rain with very high intensity.

WEICKMANN: Is this connected with lightning, a thunderstorm?

LESSMAN: Yes, normally these are thunderstorms. We have other high rainfalls which are long persisting without any electricity and the intensity per minute is very much lower. We recorded 425.5 millimeters within 22 hours and 40 minutes in September 1961.

WEICKMANN: Are there any upper-wind and cirrus cloud observations or anything like that available?

LESSMAN: No, it is completely unknown. There are no rawin or radiosonde stations in Central America, yet.

WEICKMANN: That's too bad.

FREEMAN: I might add that those of you who have spent some time in the tropics, know that he was discussing rain from a cumulus cloud that occurs quite often and lasts in one place for hours, maybe not from a single cumulus cloud, but from a colle 'on of cumulus clouds that have a finite dimension.

SOME CLIMATIC AND SYNOPTIC INFLUENCES ON DIURNAL VARIATION OF RAINFALL ON THE WINDWARD ISLANDS

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ABSTRACT

A total of 183 daily rainfall stations on six islands of the Windward group in the Caribbean is used to obtain frequency distributions of occurrence and amount of rain over a five year period. These frequency distributions are then used as a basis for a five point classification of the synoptic situation.

This arbitrary classification is used to stratify the rainfall data for a five year period from self-recording gauges on Barbados. Before sorting no significant cycles can be detected in the twenty-four hour records. After sorting according to this classification, clearcut diurnal and semi-diurnal variations in the daily rainfall are revealed.

The role of synoptic scale disturbances is illustrated and the necessity to order data in some appropriate way before subjecting it to statistical analysis is emphasized.

1. INTRODUCTION

In marked contrast to the regular and clear-cut diurnal variations in such elements as pressure and temperature, the diurnal variation of rainfall in the tropics poses a complicated problem, difficult of solution. Many factors contribute to this complexity:

1) rainfall is a discontinuous quantity in space and time with values ranging over several orders of magnitude;
2) seasonal variations from wet to dry season and space variations of climatic rainfall regimes are large;
3) amounts and frequency of rainfall vary greatly with synoptic conditions, typically some 10 per cent of the days with largest rainfall amounts contributing 90 per cent of the total precipitation [1,2]; and
4) local influences related to topography, and or convective scale motions are often dominant under certain circumstances.

Another aspect of the problem concerns the validity of applying results obtained from coastal and island records to the open tropical oceans. Recent results suggest the variations representative of the tropical oceans shows a significant semi-diurnal component with maxima near sunrise and sunset [3,4].

It seems logical that only through appropriate stratification of rainfall data with respect to climatic and synoptic parameters can meaningful information as to diurnal variations be deduced. The following reports an attempt along such lines.

2. DATA AND METHODS

Daily rainfall amounts from nearly 200 stations in the Windward Islands were collected as part of a field program carried out in 1962-63 [5]. These data are sufficient for stratification on the basis of climatic regime and synoptic conditions. Diurnal variations can be investigated only for stations with hourly amounts, and for this purpose eight recording gauges distributed over the island of Barbados were chosen. The average annual rainfall more than doubles from the driest to the wettest of these eight stations, representing therefore significantly different climatic regimes present on Barbados.

Five different synoptic categories were established primarily from the consideration of the percent of all stations which recorded rainfall on any given day, and secondarily of the average amount of rain falling each day in a given percentile range. These will be termed Modes I through V with the following definitions [3]:

- Mode I - <15 per cent of stations record rain during the day
- Mode II - 16% to 55% of stations record rain during the day
- Mode III - 56% to 75% of stations record rain during the day
- Mode IV - 76% to 85% of stations record rain during the day
- Mode V - >85% of stations record rain during the day

Intuitatively, these modes can be associated with synoptic scale controls ranging from "suppressed" through "undisturbed" to "highly disturbed", but these terms will be avoided here since quantitative measures of such parameters as divergence and vorticity cannot be presented in their support. Admittedly, the percentile limits and the number of modes chosen are somewhat subjective and arbitrary; in general, the choices were based on consideration of significant first-order discontinuities in the frequency distribution curves of per cent of stations (computed in increments of 5 per cent). Results from neighboring islands in the five-year sample used for this purpose were similar, and all stations were combined to form the basis for the final choices.

After each day was assigned to its appropriate mode, the hourly rainfall amounts for the eight Barbados recording gauges were accumulated for each mode as well as for the total sample. This was done for approximately a five year period for the months of February and March, representative of the dry season and for the months of August, September and October, representative of the wet season. The results were similar for all of the eight stations on Barbados. In consequence a single station, Seawell Airport, with the longest continuous record will be presented here for the nine year period 1954-1957 and 1959-1963 for the dry season, and the ten year period 1954-1963 for the wet season.

3. RESULTS FOR SEAWELL AIRPORT

This station is located on the south coast of Barbados in one of the drier parts of the island. The annual rainfall averages about 45 inches.

Figure 1 presents histograms of average hourly rainfall amounts and the frequency of rainfall occurrence in each hour for the 18 months dry season sample. Results for the total sample as well as for each synoptic mode are shown. Similar results for the 30 month wet season sample are presented in Fig. 2.

Before discussion of the diurnal variations, it is appropriate to note the relative frequencies with which the various synoptic modes occur, and how they change from wet to dry season. Modes II, III and IV occur with essentially identical frequencies in both wet and dry seasons. Mode I dominates the dry season decreasing by half in the wet season. Mode V is the least frequent in the dry season but dominates the wet season. Again, one's intuitive association between these modes and the degree of synoptic-scale influence on rain producing processes agree with these results.

The diurnal variations of hourly amounts or frequencies for the total sample (all modes in Figs. 1 and 2) show no distinct features in either wet or dry season with the possible exception of the suggestion of a semi-diurnal character appearing in the hourly frequencies during the dry season. Inspection of the data for the different modes, however, reveals much more distinct trends. Modes I, II and III show an easily recognizable semi-diurnal character in both amount and frequency for both wet and dry seasons. The maxima occur around sunrise and sunset with the former of greater magnitude; the minima occur near midday and during the night, with the former more distinct. Mode IV shows no clearly discernible character, a result which may partly be due to its small relative frequency. However, Mode V again shows distinct characteristics, but of a significantly different nature, especially with regard to amount. The dominant feature is the midday maximum and nighttime minimum in amount. The frequency curves for Mode V do not show distinct trends during the day.

It may be pointed out that Mode V contributes the greatest share of the total rain, particularly in the wet season, but occurs on only a small fraction of the days, especially in the dry season.

4. CONCLUSIONS

This attempt to study diurnal variations of rainfall amount and frequency after stratification of the data according to synoptic and climatic parameters shows at least partial success. The synoptic modes associated with the lower probabilities of daily rainfall in the Windward Island region show a distinct tendency for a semi-diurnal variation of rainfall amount and frequency in both wet and dry seasons. The nature of this variation is similar to that now suggested as representative of the open tropical ocean [3,4]. Conversely, the mode associated with the greatest probability of daily rainfall shows a significantly different character with a single midday maximum and nighttime minimum in both dry and wet season. It is likely that this difference is due to interactions of the local island effects and the synoptic disturbances which produce high rainfall probabilities.

The aid and assistance of Dr. Michael Garstang in the processing and interpretation of these data, and the support of this work by the U. S. Army Electronics Laboratory are gratefully acknowledged.

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FIGURE CAPTIONS

Figure 1. Histograms of average hourly rainfall amounts (open) and of frequency of hours with rain (solid) for the various synoptic modes and the total sample for Seawell Airport, Barbados during the dry season (February and March).

Figure 2. Same as Figure 1, but for the wet season (August, September and October).

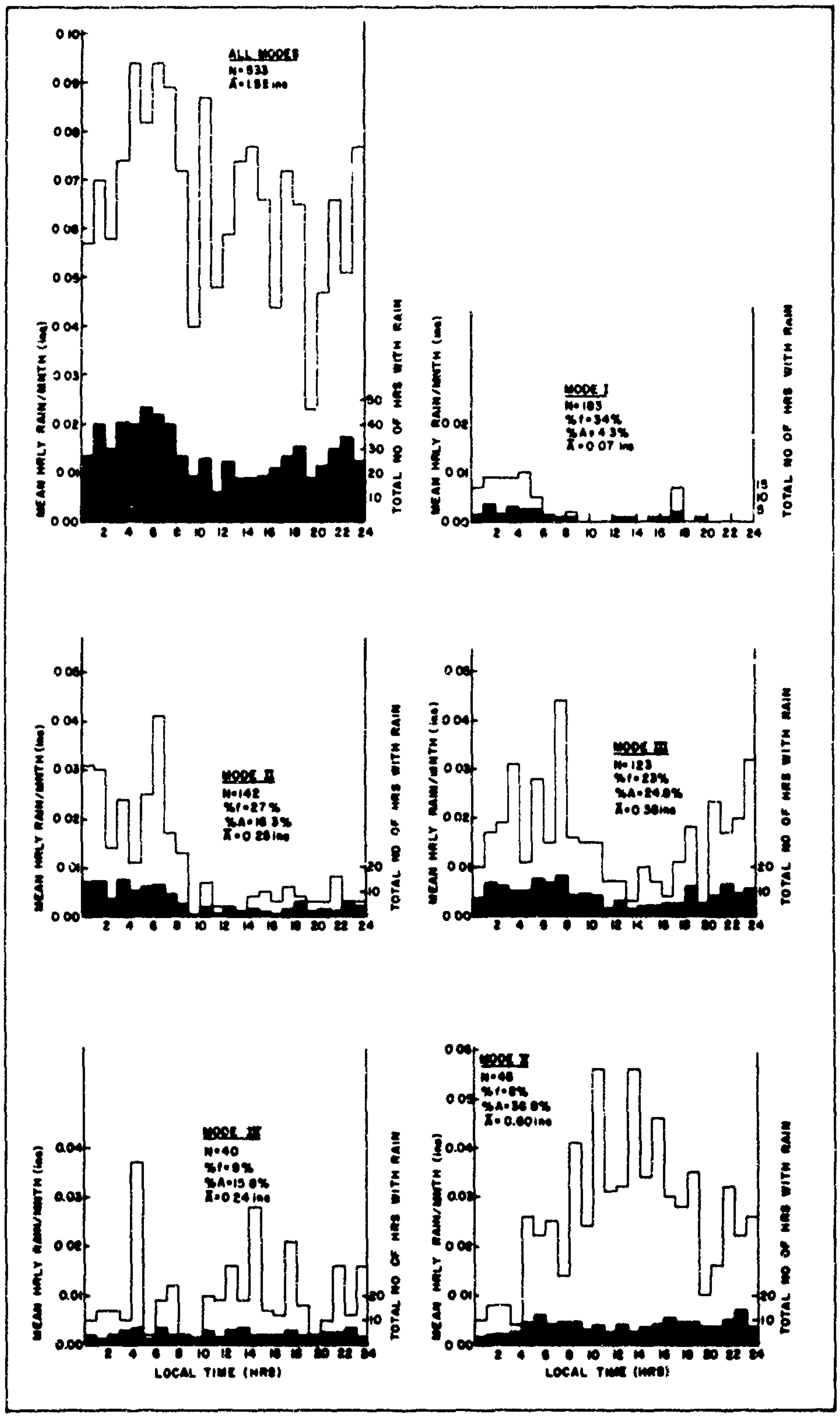
ALL MODES
MODE I
MODE II
MODE III
MODE IV
MODE V
MEAN HRLY RAIN/MNTH (ins)
TOTAL NO OF HRS WITH RAIN
LOCAL TIME (HRS)

FIGURE 1

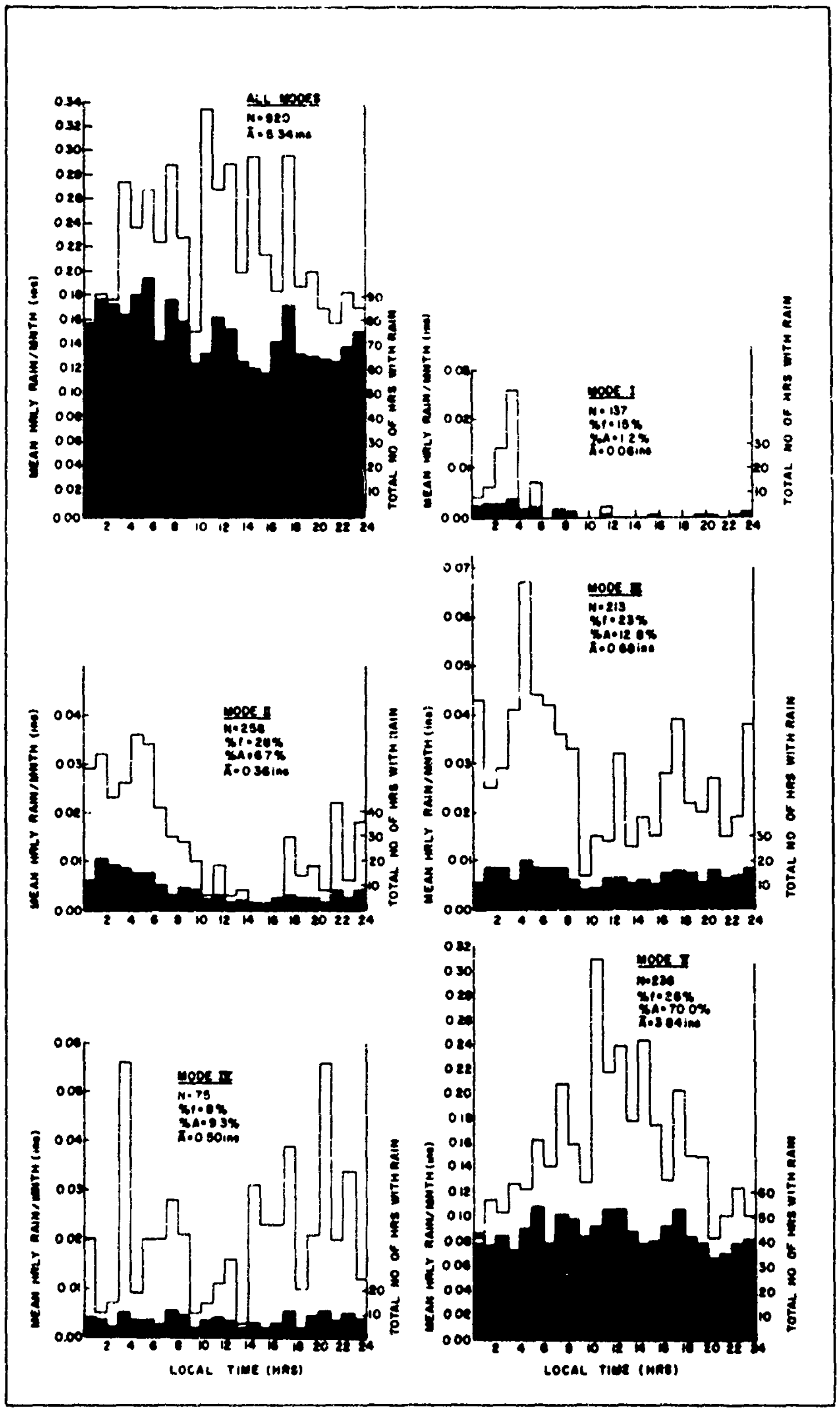
ALL MODES
N=920
MODE I
N=137
MODE II
N=256
MODE III
N=213
MODE IV
N=75
MODE V
N=236
MEAN HRLY RAIN/MNTH (ins)
TOTAL NO OF HRS WITH RAIN
LOCAL TIME (HRS)

FIGURE 2

DISCUSSION ON LA SEUR'S PAPER

GERRISH: Dr. La Seur, I think this is an exceedingly clever method of manipulating data. I am extremely impressed. Will you tell us how you selected the boundaries of these five categories, they are not linear. This must have been either a chance situation or some permutation or combination of the machine data that produced it. Possibly it could have been something that you might have thought of for selecting these intervals that brought out the data so beautifully. Do you have any comment on that?

LA SEUR: No, as I said, this is largely a subjective thing and it is perhaps fortuitous that our first intuitive choices happened to bring about such a nice stratification, or at least apparently brought about such a nice stratification in the data. It would seem now, and I see no other way than a purely subjective decision in this regard, but we could go back and simply stratify the data every 10 per cent and see at what point the apparently significant separation begins to appear. It would be ridiculous, I think, to draw a separate diurnal curve for every one percent change in the total number of stations reporting rain. On the other hand, we also know that you can't throw everything into the same pot and get any separation. Perhaps we were lucky in simply choosing five categories of roughly equal percentage intervals. If this can be done objectively, I know of no particular way of doing it. We could go back and do it for every five per cent and see at which point you begin to obtain or begin to lose benefit from the stratification. I claim nothing but luck, no deep insight in this choice, unless Mike Garstang would have a comment on that.

GARSTANG: Yes, it is basically subject to choice. You might have noticed on those curves showing number of days versus the occurrence of rain, that there were quite definite changes in slope, both in the occurrence and the amount curves.

LA SEUR: Particularly in the dry season, I think.

GARSTANG: Yes, there were these definite changes in slope and the first categorization between what we call the undisturbed versus the disturbed to simply distinquish it, this division into two could be made on this radar definite change in slope. In the sub-categorization, this becomes much more subjective. There were smaller changes of slope which we used there as guides, but again we can't really claim any quantitative method there.

KRAUS: In physical terms, if I interpreted your curves rightly, it seems that the effect of synoptic-scale disturbances over the island is enhanced during, say the middle of the day, and before I go on, is that interpretation correct?

LA SEUR: There appear to be several possibilities. It may be that this is a result of the orographic effect of the island, which although on Barbados is not large, definitely appears in a long term climatology as a maximum of rainfall at higher elevations. It seems unlikely that it can be a heating effect of the island that is enhanced during synoptic disturbances when you would expect the maximum amount of cloudiness and precipitation, but it may be an enhancement of the orographic effect.

FREEMAN. Without explanation, wouldn't you agree with the interpretation? I believe you would agree with it by default here. Do you agree with his interpretation that the synoptic disturbance seems to be greatest at noon.

LA SEUR: I would agree that there is a definite maximum frequency and amount of rainfall in midday in those days that we have classified as strongly disturbed. Whether this is due to an enhancement of the synoptic disturbance by the orographic effect of the island, or vice versa, I don't think you can decide. It may be that one could break down and further stratify according to wind direction.

COMBS: One thing bothers me and maybe Dr. La Seur will comment on it. I think we are all familiar with instruments and with the errors involved. One thing I see here is qualitative measurements of rainfall, if you will. I would just like a comment on the validity of a lot of these data, particularly the recording rain devices which, I think, can be grossly in error. I would like to know if you have given this a little thought.

LA SEUR: These eight recording gages are constantly monitored by well-qualified personnel, they are kept in excellent condition and have a potential accuracy, I would say, of the order of less than .01 inches per couple of minutes, or something like that. All of the other gages on the island are read twice daily, again by well-qualified people, and although they are mostly, I guess, the dip-stick type of gages, I would have the same degree of confidence in them as I would, say, Weather Bureau pot gages in this country. Mike, would you like to comment further on this?

GARSTANG: Yes, I would like to add that I have the same confidence as you do in the accuracy, but I would like to point out that for the particular purposes that we have been concerned with here, we are not really interested in the amount at all. It doesn't really matter of there are errors in terms of exposure or in terms of a particular gage and the wind field influence on it. This doesn't matter an awful lot. We are counting primarily occurrences rather than amounts.

LA SEUR. This is why I suggested it might be more appropriate to use frequency as the ordinate on the final graph, rather than amount. I have a great deal of confidence in the accuracy of these data. Barbados is perhaps the most densely populated land in the Western Hemisphere; there are about 300,000 people in 165 square miles.

FREEMAN They must have a high incidence of tornadoes there then.

COMBS: I would say you are very fortunate. Perhaps I should have directed my question to our people in Central America. The question was raised a few minutes ago--how come we don't have radiosonde data? Everybody pointed out there are no data and there is no instrumentation in Central America. I realize in Barbados you must have a fine crew of people and fine instruments. Let's not go into detail, but perhaps a comment from Dr. Lessman, Mr. Henry, or somebody that works in Central America on this question would be in order.

LA SEUR: I would say there are many areas in Central America much less accessible than Barbados, which is one big sugar cane plantation, densely covered by sugar cane and people; whereas in Central America you have many other problems. Dr. Lessman, or someone in Central America, would you care to comment on the data situation there?

LESSMAN: I would like to inform you that data are available especially from the Pacific side of Central America. This was the best area to develop roads and also offered the best possibilities for people to live. There we observe, during the 24 hours of the day, a maximum of rainfall frequency and amount beginning at sunset with a maximum between 2000 hours and midnight. In the morning, from 0600 to 1200 hours or to one in the afternoon, the possibility is nearly zero. But when synoptic disturbances occur from the Pacific Ocean, there is a possibility of rainfall for every hour of the day. Have I answered your question?

FREEMAN: Well, the question was essentially, are the data in that area reliable now and, if not, is there some concrete suggestion to make them reliable? You might answer this question - if you were given a radiosonde, could you operate it?

LESSMAN: Yes we could.

LA SEUR: I might make one brief comment that I would have no intention of extrapolating the results from Barbados to any other area except the immediate vicinity. Certainly I would not expect them to be necessarily applicable, say to Central America.

HENRY: I would like to answer concerning the reliability of the rainfall data in Central America. It covers the full range of reliability, I think. Some of the very well trained observers and some of the meteorological services there, I think do an excellent job. Some of your data comes from sugar plantations, from fruit plantations, from individuals who are interested in it and have kept these data. Some go back many years' from a hacienda which the great grandfather started taking these data, and this carried through son and grandson. Now, we have taken the view that these data may be a little wrong, and maybe there are some errors. I don't think you can take any other viewpoint. When we get a recorder record showing that five inches of rain fell that day, I am not going to quibble whether it is actually 4.50, 4.99 or 5.10.

FREEMAN: Well, are you going to worry about whether it was that day, or the day before, or the day after?

HENRY: Well, that might be a worry to consider, but in any event they did have a considerable amount of rain in that period, and if you take the inches, rather than the hundreths, I don't think you will have any trouble.

ORGILL: When I was in Hawaii, some of my colleagues did some work on diurnal gradation of rainfall and although they didn't stratify the data like Dr. La Seur and Garstang did, the results were quite similar to what they have come up with. There was an early morning maximum around four o'clock and then on some islands of Hawaii there was even a slight maximum in the afternoon. This early morning maximum shows up on several atolls in the Pacific. In southeast Asia there also appears to be a late afternoon maximum around 7 to 9PM and then another maximum around 2 to 4 o'clock in the morning. I believe this gives some confirmation to what La Seur said.

COBB: I wanted to make just one comment on the problem and the possibilities of data for the people here to consider. When we were in South America this last year, I personally went to Peru and talked to the people there. The WMO has given these people the equipment, in other words, reliable and good instruments to put out 711 stations in Peru, and as far as I know, they don't have two qualified meteorologists in the country. So they might have the instruments, but who are going to be the observers? I think this is also a problem in Columbia. What we need to think about is how can we train qualified observers because the instruments seem to be available, given by the WMO. The governments also have a money problem. The National Meteorological Service in Peru receives something like $1500.00 a year to publish all their records and to run their service. They get something like $1.30 for their office supplies every month. If you can t hink of some way to solve some of these problems, then I think the instruments and the possibilities of getting good observations in these areas are there.

FREEMAN: Well, thank you, I think that was a very pertinent comment.

LA SEUR: While he is getting to the microphone, I might say that the best potential source of intelligent, well-educated, easily trained, if not already trained people, would be on the island of Barbados itself. Not only are they densely populated, but most of the people are well educated and the difficulty is in finding jobs which are worthy of their technical capabilities. I have no notion as to whether one could hire them, but Barbados has a great surplus of potentially well-trained people.

LESSMAN: I would like to make this recommendation to receive better observations. You have to inspect each rainfall station at least two or three times per year and some of them can only be reached in the dry season. In this case you have to do it at the ends of the dry season, some weeks or one month before the rainy season begins in order to clean up the instrument, and to train the observer if he is still living. For example, we were not able to hold one station open more than five years because two observers were murdered. We once observed that in one station the rainfall amount was diminishing in comparison with the neighboring station and with the observations the previous years. We visited the station and saw that a banana tree had grown over the rain gage thereby protecting it from collecting rain.

THE HAILSTORMS OF LOW LATITUDES

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1. OBJECTIVE

The objective of the paper is to discuss and compare the hailstorms of low latitudes in terms of the hailstorms of mid-latitudes, since the latter have already received considerable study.

Within the extra-tropical zone between latitudes 25°N and 55°N several climatically-different hail areas have been identified. Northern Plains States and Provinces of the North American Continent provide abundant examples of swath-type hail damage, the result of large travelling storm activity; southern States provide swath and sporadic type hail damage, the result of travelling storm and single cell cumulo-nimbus development; and mountain areas of the southwest provide excellent examples of sporadic convection aided by topography.

The problem then is to show how tropical hailstorms compare in type and behaviour with those of extra-tropical lands.

2. MIDLATITUDE HAILSTORMS

North of latitude 25°N, ground hail damage has been found to take two basic patterns:

1. Sporadic patches of the order of five by five square miles.

2. Swaths, varying in length from several hundred miles to about five, and in width from about thirty miles to less than one.[1]

Sporadic hail damage may result when a situation of major convective instability develops. In this case, there are large temperature and humidity differentials between surface conditions and those at cloud heights and above, so that cumulo-nimbus clouds build to high above the freezing level and in the absence of strong winds aloft, rain or hail out in situ. This process is aided by mountainous terrain. An excellent case in point is the hail that falls on mountain peaks north of Flagstaff, Arizona in the summer time.

This type of hailstorm follows the traditional model of a vertically-developing cumulo-nimbus cloud.[2]

Swath-type hail damage on the other hand, is associated with large travelling storms. Swaths result from the coincidence of extreme instability with strong shear between surface winds and those blowing at cloud top heights and higher. In middle latitudes, shear is often produced by the presence of strong jet stream winds associated with the polar front. Hailswaths lie underneath and parallel with the fastest ribbon of the jet.

This type of hailstorm is described by Ludlam's model of a large travelling storm.[3]

Worst hail areas on the North American Continent are those of the Plains States east of the Rocky Mountains, between latitudes 25° and 55°N. These areas experience hail of the single cell and travelling storm variety. By far the larger percentage however, is attributable to the travelling storm.

Hail, predominantly of swath variety, starts to fall in southern states in March, - occasionally even in late February. It affects Oklahoma, Kansas and Missouri somewhat later, and is a feature of Nebraska and South Dakota storms by late May and early June. By July it has reached the southern Prairie Provinces of Canada.

Thus, hail is predominantly a spring and summer phenomenon in the Great Plains. The main hail belt moves northward with the seasonal progression of instability and shear resulting from (1) the gradual penetration of Gulf air into the Continent in spring and summer and (2) the gradual movement of the polar front jet stream from an average position close to latitude 30° in February to one in the latitude of the Great Lakes in July.[4]

A technique that describes instability in terms of wet bulb potential temperatures and shear in terms of 300 mb. level winds has been devised for forecasting the geographical position of damaging hailswaths.[5]

3. LOW LATITUDE HAILSTORMS

Following the work discussed in the paper "Hailstorms from Tropics to Midlatitudes"[6] a tabulation was made of documented hailstorms from low latitudes, These have been studied individually in recent months.

DOCUMENTED HAILSTORMS OF LOW LATITUDES

Caribbean

No.	Place	Lat. (approx)	Date	Time	Duration	Size	Notes
1.	Loiza & Guraba Valleys, Puerto Rico	18°N	12 May 1905	1400			Narrow swath, 30 mi. long. Accompanied by "whirlwind". Damage to uncut tobacco.
2.	Loiza & Guraba Valleys, Puerto Rico	18°N	13 May 1905				Less extensive than storm of previous day.
3.	Santurce, Puerto Rico	18°N	6 May 1926	2030	15 min.	Hen egg	Hail fell on residential area outside San Juan.
4.	Morovis, Puerto Rico	18°N	29 Sept. 1955	1305	10 min.	Less than pea	Thunderstorm started 12:45 p.m. Damaged trees & power lines.
5.	Utuado, Puerto Rico	18°N	12 Feb. 1957	p.m.	3 min.	Corn kernel	$15,000 damage to tobacco crop.
6.	Morovis, Puerto Rico	18°N	8 Nov. 1963	1450	3 min.	Less than pea	Damage to sugar cane and banana crops.
7.	St. Kitts	17°N	24 Nov. 1963	0210	10 min.	3 inch diam.	South and North coasts affected.

No.	Place	Lat. (approx)	Date	Time	Duration	Size	Notes
8.	Miami, Florida	26°N	29 Mar 1963	1130	20 min.	1-3" diam.	Short, narrow swaths.
9.	India, Seoni, Betul & Shahdol Districts	20-24°N	19 Mar 1962	1600-2000	3-30 min.	Small berry to marble	Damage to Mangoes
10.	Australia, Thangool, Queensland	24°S	16 Oct. 1964			Small	Swath. 15 mi x ½ mi Hailstones 2 inches deep.
11.	Hawaii, Hilo	20°N	20 Mar. 1964	1530-1550	20 min.	¼ inch diam.	First hail to fall in downtown area.
12.	Honolulu	20°N	4 Feb. 1965				"Heavy hail."

Hailstorm studies selected for inclusion in this text are numbers 6, 7, 9 and 10 from the tabulated list.

Diagrams illustrating the storm studies are of sketch rather than accurately-scaled map variety.

4. FOUR TROPICAL HAILSTORM STUDIES

6. Morovis, Puerto Rico. 8 November 1963.

This storm was described by the official Weather Bureau observer at Morovis, Puerto Rico. Morovis is in a cultivated area of broken volcanic hills.

A thunderstorm started at 1:30 p.m. on 8 November 1963. Cumulo-nimbus clouds built up to great heights to the southwest and southeast of the observing station. "Hard" rain fell at 2:30 p.m. followed by hail at 2:50 p.m. The hailstorm lasted for about 3 minutes. Hail was close to pea size. It is reported that people picked up handfulls of small hailstones in the streets and licked them as they would ice cream. Sugar cane and banana crops were damaged.

As far as can be judged, this was a case of sporadic hailfall, - no evidence of a swath was found. From the data reported, wet bulb potential temperatures at the time of the storm must have been in excess of 74°F. Surface winds were easterly 25 knots. 200 and 300 mb. winds were westerly, 25 knots. (This relatively low wind-speed would in any case be marginal to swath formation.5) The morning sounding for San Juan about 25 miles away, showed a great deal of moisture in the lower layers and high conditional instability, once the shallow ground and trade wind inversions were broken down. This would be easy to achieve if afternoon temperatures were around 85°F.

The Pseudo Adiabatic chart for 1200z on 8 November 1963, as well as a surface analysis for the same hour showing 200 mb. winds in the area of Puerto Rico, are presented in Figure 1.

7. St. Kitts. 23 November 1963.

This storm is one of the best documented and one of the most interesting found thus far. Informative details were gathered from the Church Register at Middle Island, from the Weekly Democrat on November 30, 1963 and with the help of the Basse-Terre U. S. Weather observer, from inhabitants of the Island themselves.

An electrical storm started at about 8 p.m. on

November 23, 1963. Lightning was incessant, sheet and forked, until after midnight. Everyone remarked on how cool the evening was. Then between 0200 and 0230 LST on the 24th, people were wakened by large hailstones falling on house roofs. Stone sizes varied from one to three inches in diameter.

The hailstorm lasted about 10 minutes. It affected sugar estates in the Tabernacle and Phillips areas of the northeast coast and halfway Tree, Middle Island, Old Road Town and West Farm Estate, on the Southwest shore. Since the storm happened at night, no information has been found concerning hailfall on the untenanted higher sections of the Island away from the coasts. Thus the damage pattern at the ground remains unresolved, and could have been either swath or patch.

St. Kitts lay in an unstable trough associated with a surface occlusion. (Dry bulb temperature 73°F. and dewpoint 69°F.) Surface winds were easterly, 200 mb. winds westerly at 50 knots, with sharp shearing across the trough.

The sounding for St. Maartens - closest to St. Kitts - six hours after the storm, demonstrated considerable humidity in the lower layers and a lower freezing level (11,800 feet) than is usually encountered in this area. By 0800 on the 24th, the Trade wind inversion was once again established, so that the sounding does not reflect the degree of instability that must have existed at the time of the storm. Figure 2.

If this storm was of swath variety, - and all the requisite ingredients were present, - it must have meant that hail fell <u>over the sea</u> in this instance <u>as well as on sea coasts</u>.

<u>9. Thangool, Queensland, Australia.</u> 16 October 1964.

This storm was described in a Rockhampton newspaper and later documented by the Australian Meteorological Office. It occurred in the afternoon of 16 October 1964 and left a trail of damage about 15 miles long and half a mile wide, a few miles north of Thangool. (Total dimensions of the swath may have been greater than this, since crop losses other than to grain, were not reported.) Small hailstones fell to a depth of about 2 inches. The swath lay from southwest to northeast.

A weak ridge extended north-south over the east coast of Australia between a large high pressure system centered in the western Pacific and a shallow elongated trough stretching from the Gulf of Carpentaria through Central Queensland to New South Wales.

Wet bulb potential temperatures exceeded 66°F, and since winds were light northerly at the surface, westsouthwest at 50-80 knots at 300 mbs. there was moderate shear.

Thus, conditions in the vicinity of Thangool were similar to those of similar latitudes in the northern hemisphere e.g. Florida when hail has occurred. Again, the hail swath was parallel to the fastest ribbon of the 300 mb. jet. Figure 4.

10. Central India. 19 March 1962.

This storm was observed and documented by personnel of the Indian Meteorological Service. Hail damage occurred in the Shahdol, Seoni, and Betul districts of central India during the late afternoon and evening of 19 March 1962. (1610-2015 LST). Storms varied in duration from 3 to 30 minutes. Hailstones were reported to be from small berry to marble size.

Warm moist air was brought into the heart of the Continent on 18 and 19 March 1962 in the circulation of a high pressure system centered on the Bay of Bengal.

Thus, by the afternoon of the 19th, there was humidity as well as heat in the Nagpur/Allababad area. Wet bulb potential temperatures were in the seventies, and 300 mb. winds about 70 knots from the southwest.

All three districts are in hilly country south of the Narbada River Valley. In favourable conditions such as these, topography could be expected to play a major part in thermal development.

Since there is no complete record of the pattern of hail damage at the ground, it is not possible to determine conclusively whether this was a case of organised swath-type hail, or whether the hail fell in unorganised clusters. At all events, it occurred in the general direction of the upper level winds (southwest - northeast) and it seems quite possible that all 3 areas of known damage were actually part of one loosely-organised broad swath. Figure 3.

5. METEOROLOGICAL CONSIDERATIONS AFFECTING HAILSTORM FREQUENCY AND INCIDENCE IN LOW LATITUDES

From the tabulated list of tropical hailstorms it might be inferred that hail at low latitudes occurs only in fall, winter and spring.

Hailstorm incidence from the list of
documented hailstorms of low latitudes

J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
-	2	3	-	3	-	-	-	1	1	2	-

An as yet undocumented record of hailstorms has recently been received from Costa Rica however, that shows hailstorm frequency in that mountainous country only about 10°N of the equator, to be highest in April, May, August and September.

Hailstorms in Costa Rica 1960-1964

J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
-	-	-	6	3	1	3	3	7	1	-	-

Thus there is no unanimity in the timing of tropical hailstorms.

Of the four storms analysed in the text, the first was a case of sporadic convection; the second occurred at night and may have produced either patch or swath-type hail; the third produced a short, narrow swath; and the fourth appears to have resulted in a loosely organised swath of considerable dimension.

Thus all the categories of hailstorms identified at higher latitudes have been found to occur at low latitudes as well. They are found less frequently, however. Some of the reasons for the low frequency of hailstorms in the tropics may be the following:

(1) A convective-type hailstorm requires orographic lift for triggering. Caribbean Islands lying close to sea-level are incapable of providing this lift and therefore are precluded from experiencing hail of the purely sporadic convective type. Hail reports have already been received however, from most of the islands that possess hill and mountain topography 3000 feet or above.

(2) A swath-type hailstorm in the tropics labours under a serious disadvantage. Whereas in midlatitudes, shear is provided largely by jet stream winds at the 300 mb. level[5] and swaths lie underneath and parallel with the fastest ribbon of the jet, in tropical latitudes, due to the greater height of the tropopause, jet stream winds blow at closer to 200 mb. This in turn means that clouds must build to greater heights than is usual in midlatitudes, before jet stream winds can affect their topmost sections.

(3) Not only is hail formation hindered by the

increased height of the tropopause in the tropics and the increased height of the jet stream winds blowing below it, but due to year-round high temperatures at the surface, the freezing level is also correspondingly high.

Thus storms need to be particularly intense at low latitudes to be eligible for hail production.

In summary, it is hypothesised that the conditions under which hail occurs are similar in low and midlatitudes but that the requisite degrees and combinations of instability and shear occur less frequently in the tropics than further north.

6. CONCLUSIONS

Summarising, from data assembled to this time it is concluded tentatively that hail in the tropics:

a. occurs sporadically and in swaths. The swaths appear to be shorter and narrower than those of midlatitudes.

b. falls principally in afternoons but has been known to happen after midnight.

c. occurs at sea level as well as over higher ground.

d. occurs in summer months in certain areas; in fall, winter and spring months in others.

e. occurs under the same atmospheric conditions in low as in midlatitudes.

f. occurs less frequently than in midlatitudes because the requisite degrees and combinations of atmospheric conditions occur less frequently at low than at midlatitudes.

g. varies in time of occurrence, depending upon the varied meteorological and topographical characteristics of individual areas.

More data are needed to confirm these statements, but they will become available as observers are alerted to note hail occurrence in addition to the occurrence of precipitation and thunderstorms. A further requirement is for cloud-top height measurements, in order to know to what height clouds build above the freezing level in actively convective conditions, and what wind speeds are encountered at these heights.

7. ACKNOWLEDGEMENTS

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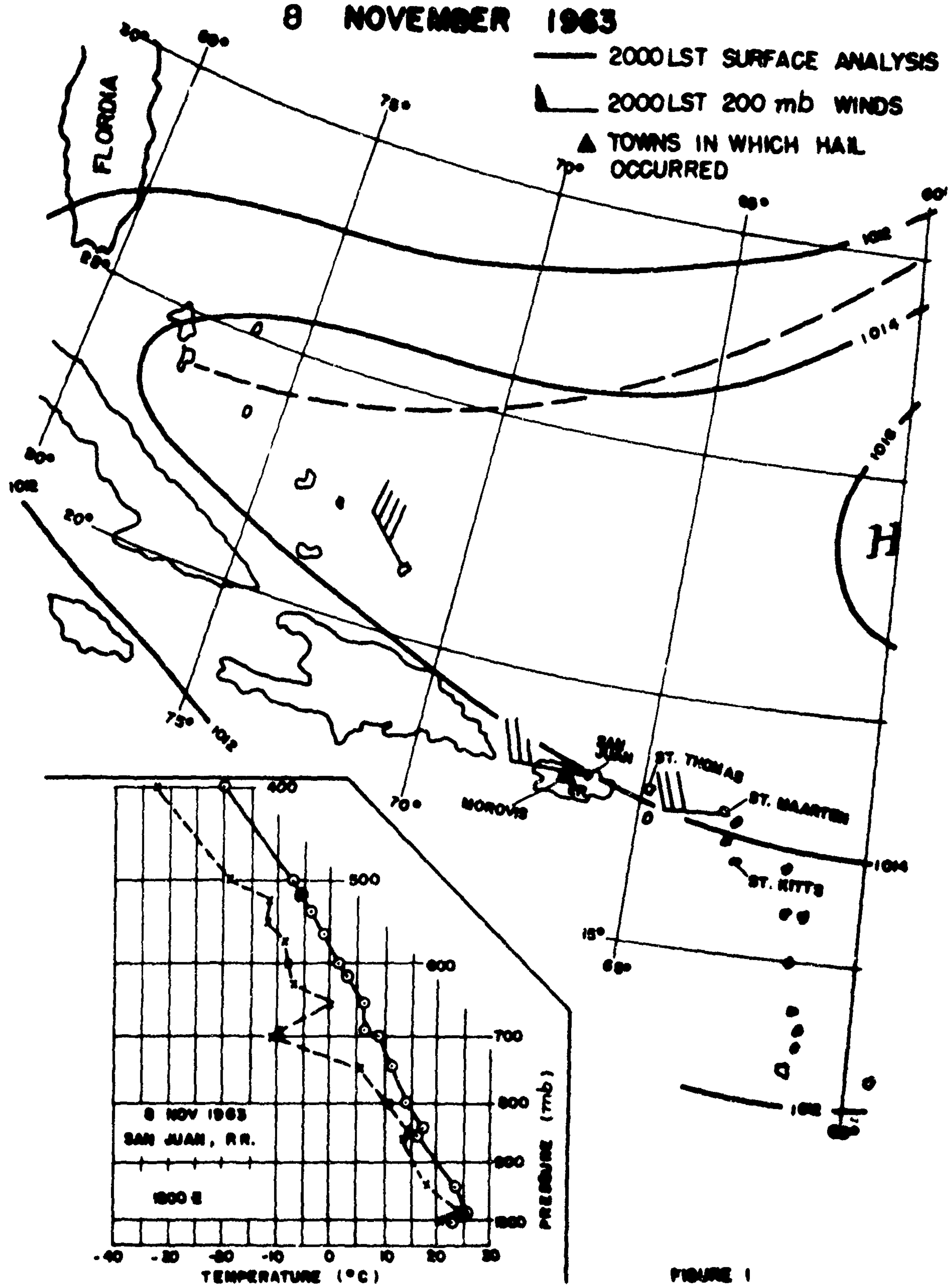

8 NOVEMBER 1963
2000 LST SURFACE ANALYSIS
2000 LST 200 mb WINDS
TOWNS IN WHICH HAIL OCCURRED
FLORIDA
1012
1014
1016
H
SAN JUAN
ST. THOMAS
ST. MAARTEN
ST. KITTS
MOROVIS
8 NOV 1963
SAN JUAN, P.R.
PRESSURE (mb)
TEMPERATURE (°C)

FIGURE 1

24 NOVEMBER 1963

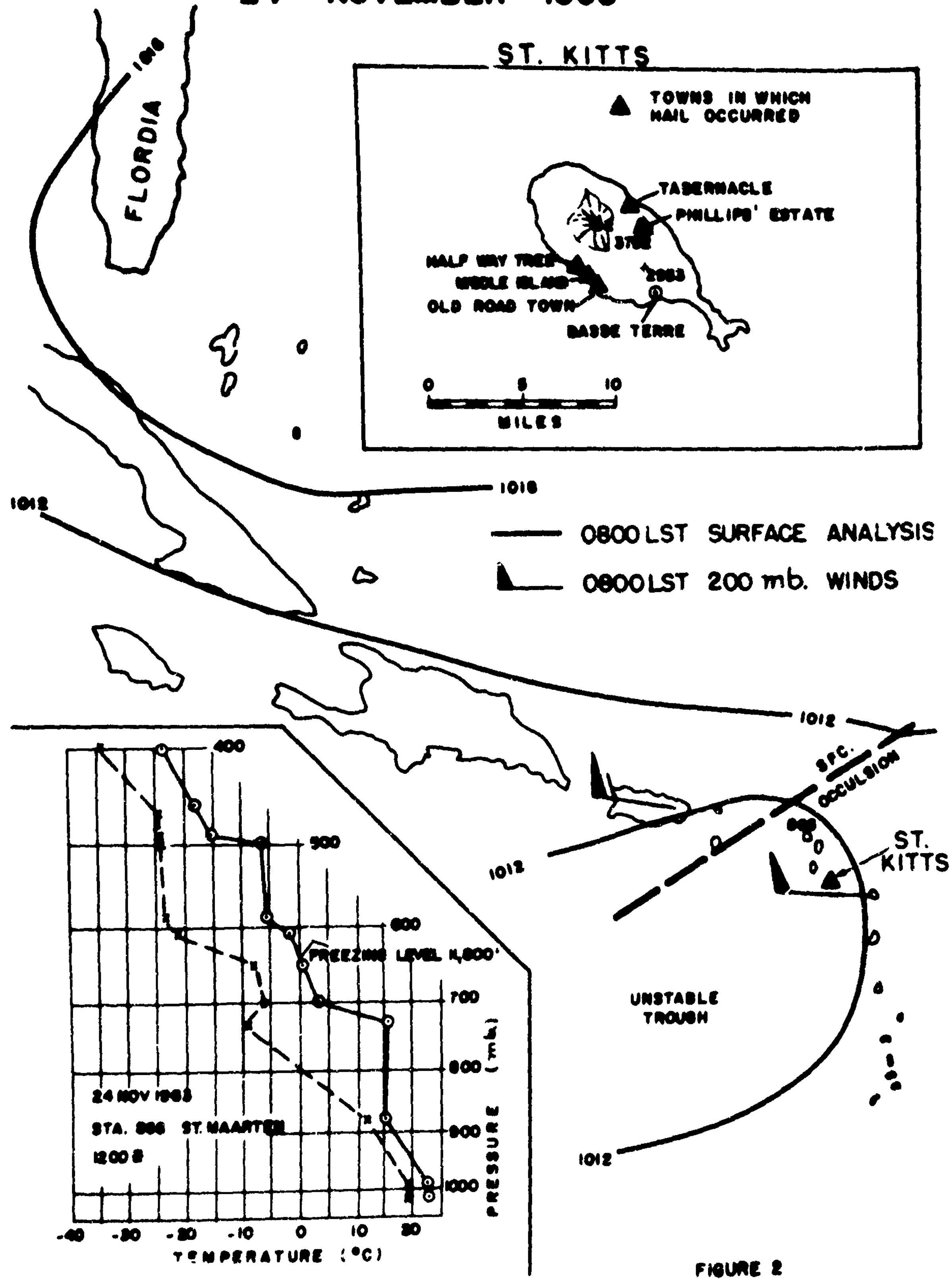

FIGURE 2

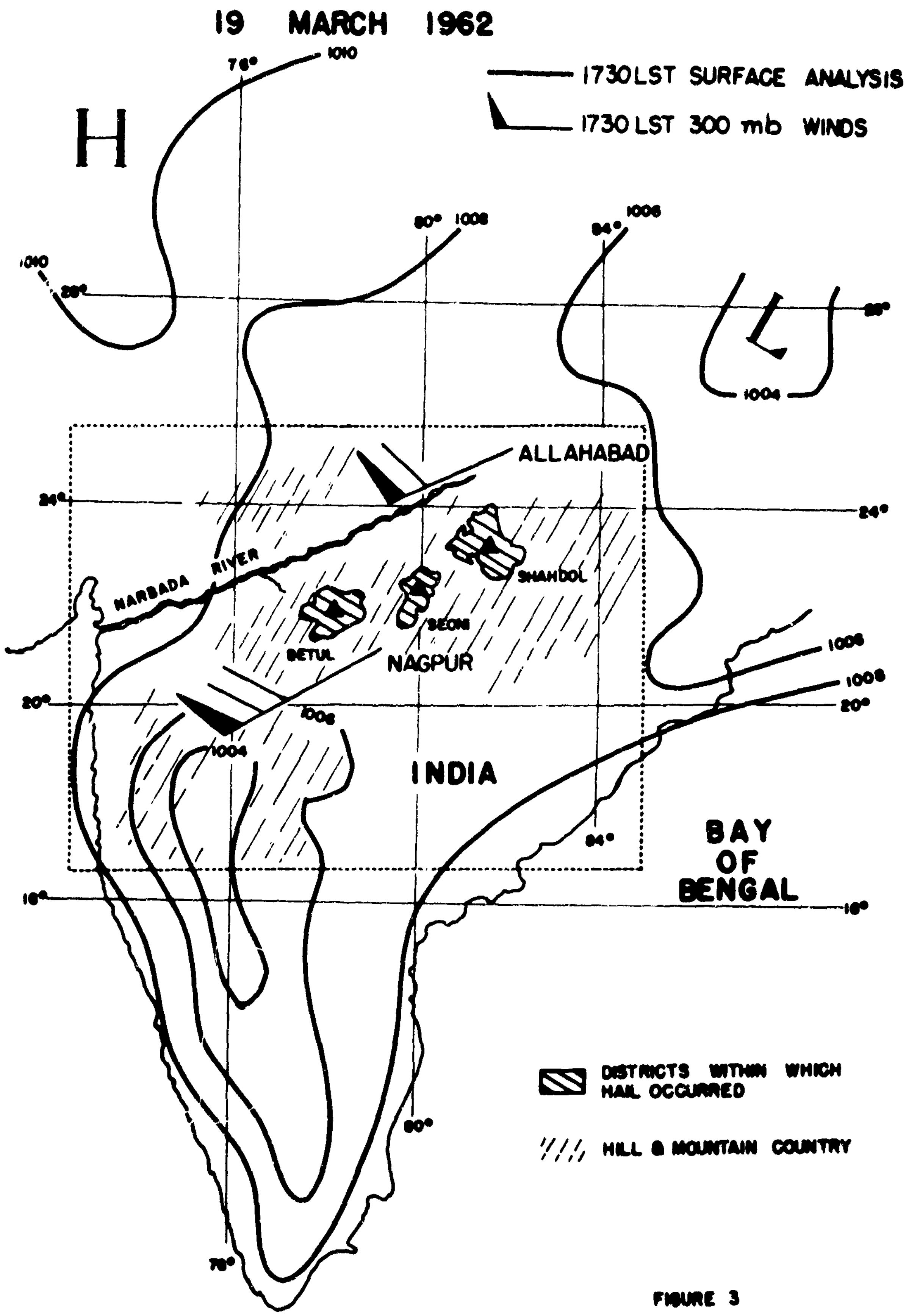
19 MARCH 1962
1730 LST SURFACE ANALYSIS
1730 LST 300 mb WINDS
H
L
1010
1008
1006
1004
ALLAHABAD
NARBADA RIVER
SHAHDOL
SEONI
BETUL
NAGPUR
INDIA
BAY OF BENGAL
DISTRICTS WITHIN WHICH HAIL OCCURRED
HILL & MOUNTAIN COUNTRY

FIGURE 3

16 OCTOBER 1964

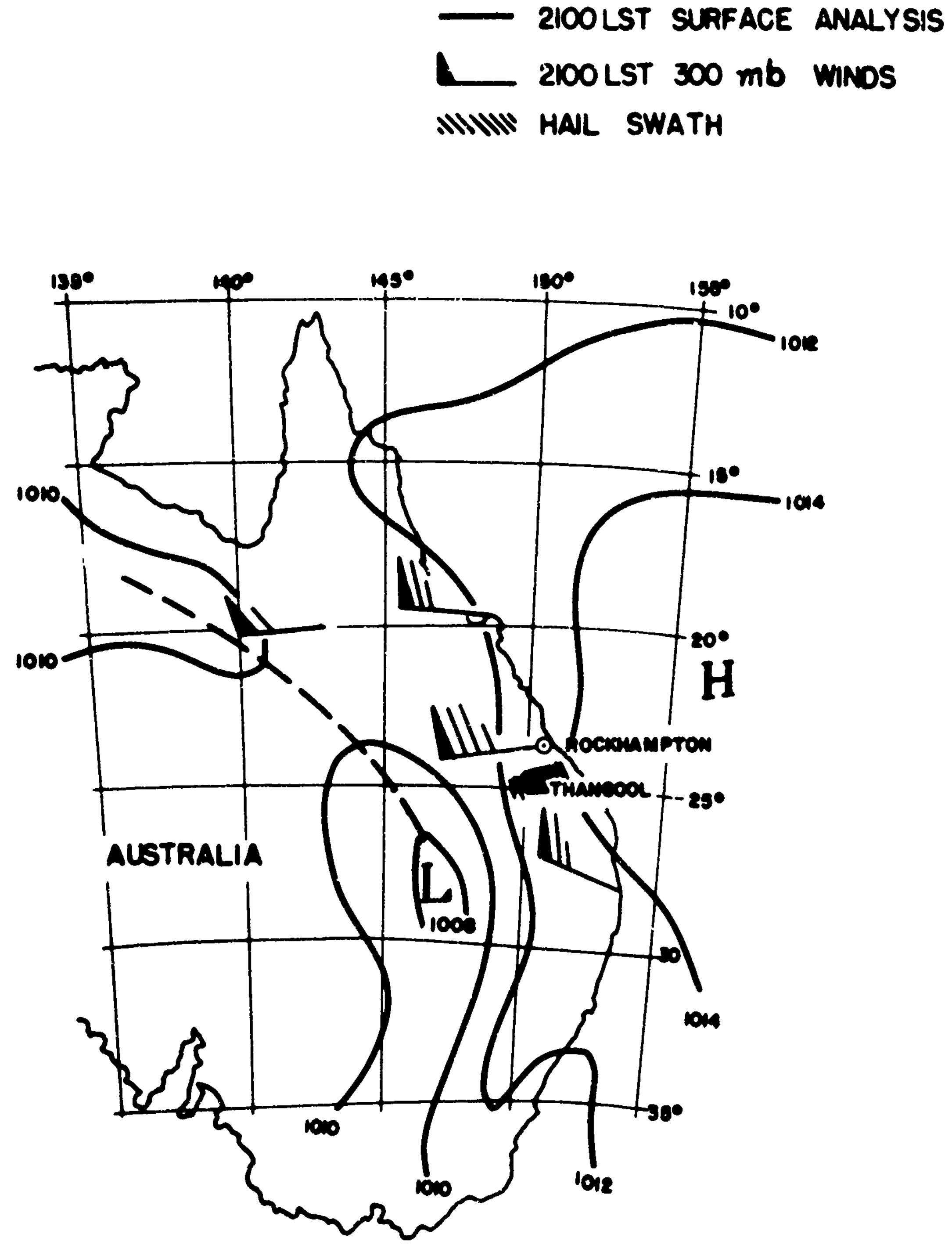

FIGURE 4

DISCUSSION ON FRISBY'S PAPER

GOLDMAN: Is there any accepted explanation why vertical wind shear is necessary for hail? Are you saying that it is one of the necessary conditions, that is, a dynamic explanation?

FRISBY: To go back quite a long way in this whole argument, I would have to say that as far as the large traveling storms are concerned, I have been guided almost entirely by the work of Dr. Frank Ludlam. I have used his model of a large traveling storm as my model for hail that occurs in coincidence with that type of storm. Now I think in terms of the overhang type of storm, the storm with the forward lean, and the storm which is purely a case of updraft, this I think of as the upright model. Therefore, I have two models in my mind and two types of hail storms always to consider - the hail storm which results as the case of the forward lean and the other that occurs from the single cell cumulonimbus with its vertical development.

GOLDMAN: I believe Dr. Ludlam's storm is involved with so-called giant hail which is about two inches in diameter, or greater. I think you mentioned pea-size hail. Are you saying, then, that the pea-size hail is from the purely vertical thing, or what's the relationship between the shear and the size of the hail?

FRISBY: It seems that in the cases that I have considered, particularly if we think of the Morovis case, the pea-size hail, with no evidence for thinking that this was the result of a large traveling storm, was the result of vertical cloud development, but now I think its very likely that this particular storm, the St. Kitts one, was swath-type hail from a large traveling storm. There's still more investigation that has to be done on this. I want to find out whether there is any evidence upwind to decide whether this was a large traveling storm, or not. I think it is very likely that I can get some more evidence from San Juan, Puerto Rico in regard to this particular one. But from the values that were received, I think it was a swath-type storm and certainly in this particular case, the size of the hail was very large. So, it may be that there is a distinction that can be made here. I don't know yet, I think it is likely.

KOTESWARAM: I was very much interested in that map for the Indian storm which you have illustrated. From my own experience I can say that every year in Northern India, and particularly, say, north of about 18 degrees north, the hailstorms are quite frequent during the spring months. That's the three months which are mentioned in your work, and in fact there is a very well documented case which was discussed in a paper by Mull and Kulsreshta in a recent article in the Indian Journal of Meteorology and Geophysics. In fact that was a case in which hail damaged an aircraft flying through a hail shower. I saw the pilot who landed the aircraft and he was bleeding all over. The hail was reported more than about 3 or 4 inches in diameter. Apart from that, the question of hail in the tropics did not interest me in this way. All of these hail events occurred in the tropics, we agree about that as far as I know what to call the tropics, but the problem seems to be that hail is occurring in all these areas in conditions which are generally extra-tropical in nature. You can't simply say that this is tropics in that particular way. There is no difference between the type of hail which is occurring and the type of synoptic situation in which hail is occurring in the lower latitudes as compared to the upper latitudes because in all these cases you want some westerly winds, you want a strong westerly wind, a jet stream, and that kind of thing. Once this extra-tropical atmosphere is removed, when you get purely easterlies in the upper levels, one doesn't get the hail at all. Therefore, I don't quite feel justified in describing all these things as hail in the tropics. Could you think of modifying this by saying "hail in the tropics during extra-tropical situations" or some such thing?

DIURNAL AND SEMI-DIURNAL VARIATIONS OF SENSIBLE AND LATENT HEAT EXCHANGE, CLOUDINESS AND PRECIPITATION OVER THE WESTERN TROPICAL ATLANTIC

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ABSTRACT

Twenty-three days of data were collected at a fixed ocean station east of Barbados during the 1963 program. These data are combined with a similar set gathered in 1957 to investigate diurnal and semi-diurnal variations over the open tropical oceans.

A marked diurnal variation in sensible heat transfer is obtained but a less defined diurnal change is found in latent heat transfer. These results indicate that the spatial distributions of sensible heat transfer obtained by previous workers are likely to be in error.

A distinct semi-diurnal variation in cloudiness and precipitation is obtained for this open ocean station. Consistency in results of the two observing periods as well as consistency of various measurements leaves no doubt about the existence of such regular cycles of cloudiness and precipitation over the open ocean.

The relationships between these cycles and the diurnal variation in heat transfer is not yet clear. At least two other mechanisms must be considered, namely radiation and the atmospheric tide.

1. INTRODUCTION

Forty-six days of observations were gathered on two separate cruises of a research vessel in the ocean region immediately east of the island of Barbados. Both cruises took place during the months of August and September. In each case the ship was on a fixed station for twenty-three days. During the first cruise in 1957 it was located near 11°N 52°W, and in the second in 1963 in connection with the U. S. Army sponsored Tropical Weather Research Project on Barbados, near 13°N 55°W, or 275 miles east of Barbados.

Wet and dry bulb temperatures and wind speed were recorded at a single level some five meters ahead of the bow of the vessel, as shown in Fig. 1. Sea surface temperature was recorded at 10 cm the same distance ahead of the vessel. Mean values for each quantity could then be obtained over a time period of one hour. Considerable care was taken to avoid extraneous effects. Where this was not possible, a measure of the effect was obtained and a correction applied.

A measure of atmospheric stability in the first ten meters could be obtained from these hourly mean values by means of the Richardson bulk number. The range of stability obtained is shown in Table 1 below:

TABLE 1. STABILITY
(1000 observations)

Unstable				Stable	
-0.325	<	R_B	<	+0.025	96% of the time
-0.040	<	R_B	<	+0.020	60% of the time

where
$$R_B = \frac{g}{T} z \frac{\Gamma_h}{\Gamma_m 2} \frac{\bar{\theta}_a - \bar{\theta}_s}{\bar{u}_a{}^2} \quad [1]$$

Under these circumstances logarithmic profiles are closely approximated and the most restrictive assumptions of the bulk aerodynamic equations are most nearly met. In consequence, these equations were used to compute latent and sensible heat transfer.

$$LE = Q_e = \rho L C_D [\bar{q}_o - \bar{q}_a] \bar{u}_a \quad [2]$$

$$Q_s = \rho C_p C_D [\bar{\theta}_o - \bar{\theta}_a] \bar{u}_a \quad [3]$$

where Q_e and Q_s are respectively, the eddy vertical transport of latent and sensible heat and are directly proportional to the differences berween the mean specific humidity and mean potential temperature measured at the surface and at some point above the surface, multiplied by the mean wind speed measured at the same point above the surface.

It is important to note that because of the critical role that wind speed plays in equations [1], [2] and [3], <u>the higher the heat transfer, the more accurately the formulae predict it</u>. In fact, if the bulk aerodynamic equations can be used at all, they can probably be best applied to conditions prevailing over the tropical oceans. However, it is also quite clear that the drag coefficient, C_D, is not a constant but is a function of, at least, height (z), wind speed (u) and stability (R_B). By applying considerations outlined by Monin and Obukhov [1954], it can be shown that

$$C_6^* - C_{11}^* \left(\frac{\bar{u}_{11}}{\bar{u}_6}\right)^2 = a\, R_B [C_6^{*\,3/2} - C_{11}^{*\,3/2} \left(\frac{\bar{u}_{11}}{\bar{u}_6}\right)^2] \quad [4]$$

where C* is the drag coefficient for neutral or adiabatic conditions and the subscripts 6 and 11 refer to heights (in meters) above the sea surface. A linear dependence of the drag coefficient upon height and wind speed under adiabatic conditions was assumed using values obtained by Deacon and Webb [1962]. In practice [4] was approximated and the contribution of the term in brackets on the right hand side was neglected. Figure 2 shows the functional relationship between the remaining terms of the above expression. Based upon these considerations, the drag coefficient was then computed from

$$C_6 = (1.46 + 0.07\ \dot{u}_6 - 4.2\ R_B) \times 10^{-3} \qquad [5]$$

2. DIURNAL CHANGES IN SENSIBLE HEAT TRANSFER

Equations [2], [3] and [5] were then used to compute latent and sensible heat transfer for every hour of the forty-six day period. The diurnal variation of the difference between the sea and air temperature is shown in Fig. 3. Figure 4 shows consequent diurnal variations in sensible heat transfer. The maximum range in Q_s is greater than 30 cal $cm^{-2}day^{-1}$, with mean values at 10 cal $cm^{-2}day^{-1}$ for 1957 and 18 cal $cm^{-2}day^{-1}$ for 1963.

Previous investigators have all used fixed GMT times on which to base their calculations. Jacobs [1951] used 1200 GMT which means that values of Q_s in the Atlantic are computed for daylight hours and for the east Pacific for the night-time hours.

Figure 5 shows the mean annual distribution of Q_s obtained by Jacobs [1951]. Negative values are obtained in the tropical Atlantic, whereas positive values are obtained in the eastern tropical Pacific. The distribution obtained by Budyko [1956] is shown in Fig. 6. While Budyko does not specify what observing time was used, it appears to be 0000 GMT since a reversal of Jacobs' distribution is obtained.

Corrections cannot easily be applied to these maps. Jacobs' computations were based upon monthly mean values of sea and air temperatures [Atlas of Climatic Charts of the Oceans, 1938]. For example, for August at the ship location, i.e. about 8 a.m. LST, Jacobs gives the sea-air temperature difference as -0.39°C, i.e. the mean air temperature is warmer than the mean sea temperature. The twenty-three day mean values for the ship location were, respectively, +0.47°C in 1957 and +0.52°c in 1963. These errors are due both to radiational heating and the heating effect of the ship. Such errors, therefore, do not reflect real diurnal changes and no purpose would be served by correcting these values using the above diurnal march of sensible heat.

3. RELATION BETWEEN SENSIBLE HEAT TRANSFER, CLOUDINESS AND PRECIPITATION

We might expect that if the diurnal march of sensible heat flux presented is valid, one should be able to detect its effect upon other parameters in the atmosphere. There are, however, at least two other mechanisms with diurnal or semi-diurnal cycles:
(a) radiation [e.g. Kraus, 1963],
(b) the atmospheric tide [e.g. Shibata, 1964].

3.1 Semi-diurnal variation of cloudiness

Variations of low cloud in oktas of sky covered is shown in Fig. 7. Here we clearly have a semi-diurnal oscillation. It is suggested, however, that the primary maximum and minimum reflect the diurnal variation in the transfer of sensible heat. As shown in Fig. 8, the divergence-convergence cycle of the atmospheric tide at 3 km shows a close correlation to the cloudiness cycle.

3.2 Semi-diurnal variation of precipitation

Precipitation echo frequency is shown in Fig. 9 which reflects and confirms the distribution of cloudiness. Similarly, the occurrence of showers at the vessel, as can be seen from Fig. 10, shows the same general distribution.

As in the case of cloudiness, the diurnal variation of sensible heat can be called upon to account for the primary maximum and minimum that occur during the early morning and midday hours.

4. DIURNAL VARIATION OF LATENT HEAT TRANSFER

Because the diurnal variation of saturated specific humidity at the sea surface and of specific humidity at 6 meters is either small or non-existent, the diurnal variation of latent heat, as shown in Fig. 11, is also small in comparison with the mean daily flux of latent heat. Because of this the maximum range, which exceeds 50 cal $cm^{-2}day^{-1}$, is only a small fraction of the total amount of heat transfer involved. However, it should be noted that the semi-diurnal variation in cloudiness implies a semi-diurnal variation in latent heat release.

5. CONCLUDING REMARKS

Large scale distributions of sensible heat in tropical regions can be significantly affected by diurnal changes in this quantity. Valid results can only be expected if full account is taken of the diurnal variation.

Lack of adequate data over the tropical oceans virtually precludes any direct approach to this problem. It is likely that carefully made measurements at selected locations is the only way to solve the problem.

The diurnal variation of sensible and latent heat play a definite role in the diurnal variation of cloudiness over the open tropical oceans. The fact that semi-diurnal variations in cloudiness and precipitation have been established suggest, however, that other mechanisms must also be considered. The relative importance of these various mechanisms remains to be established.

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FIG. 1. Instrumentation on the pulpit. The tube-buoy can be seen on the sea surface with the counter weight system extending back to an arm on the starboard side. The gimballed anemograph is located 6.0 meters above the sea surface, together with the wet - and dry-bulb resistance thermometers housed within the circular radiation shields.

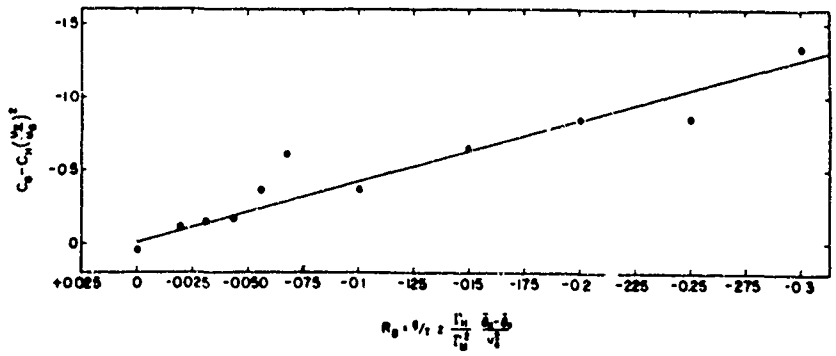

FIG. 2. Departures from the neutral value of the drag coefficient at 6.0 meters as a function of stability expressed in terms of the bulk Richardson number.

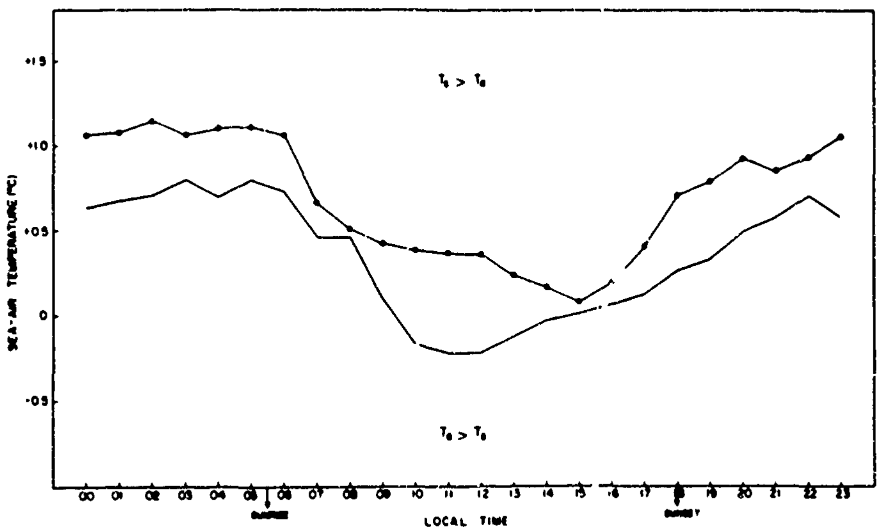

FIG. 3. Diurnal march of sea-air temperature difference (C) constructed for 1957 (——) and 1963 (-o-).

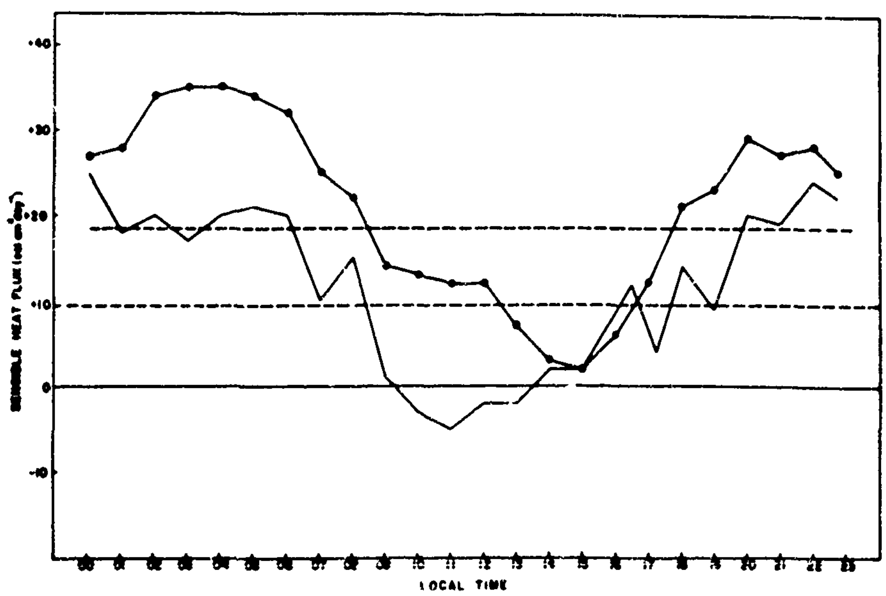

FIG. 4. Diurnal march of sensible heat transfer (cal $cm^{-2}day^{-1}$) between the ocean and the atmosphere based upon the 1957 (——) and 1963 (-o-) ship data.

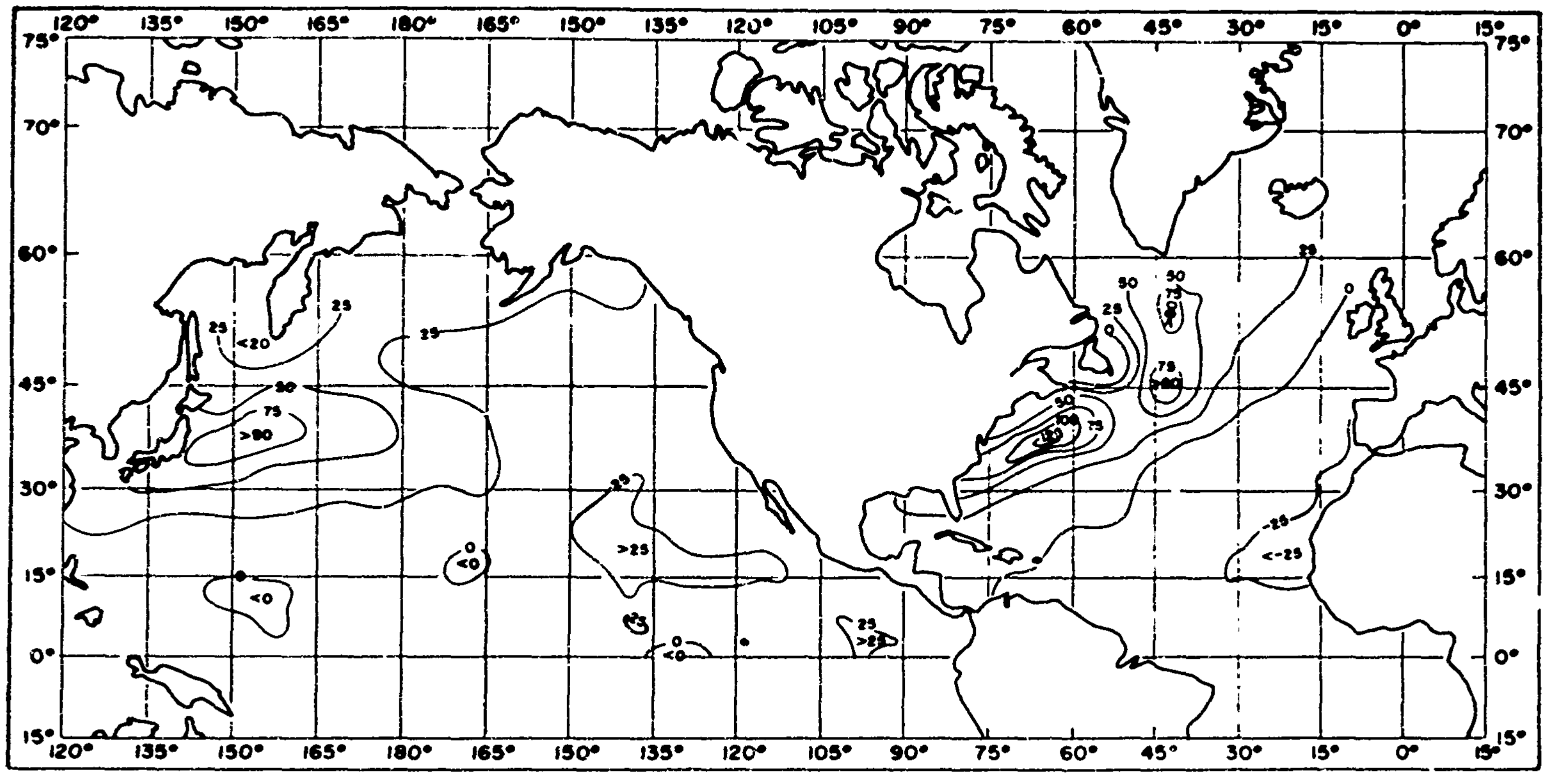

Fig. 5. The mean annual rate of exchange of sensible heat Q_s, between sea and air over the North Atlantic and North Pacific, expressed in gram calories per square centimeter per day. After Jacobs [1951].

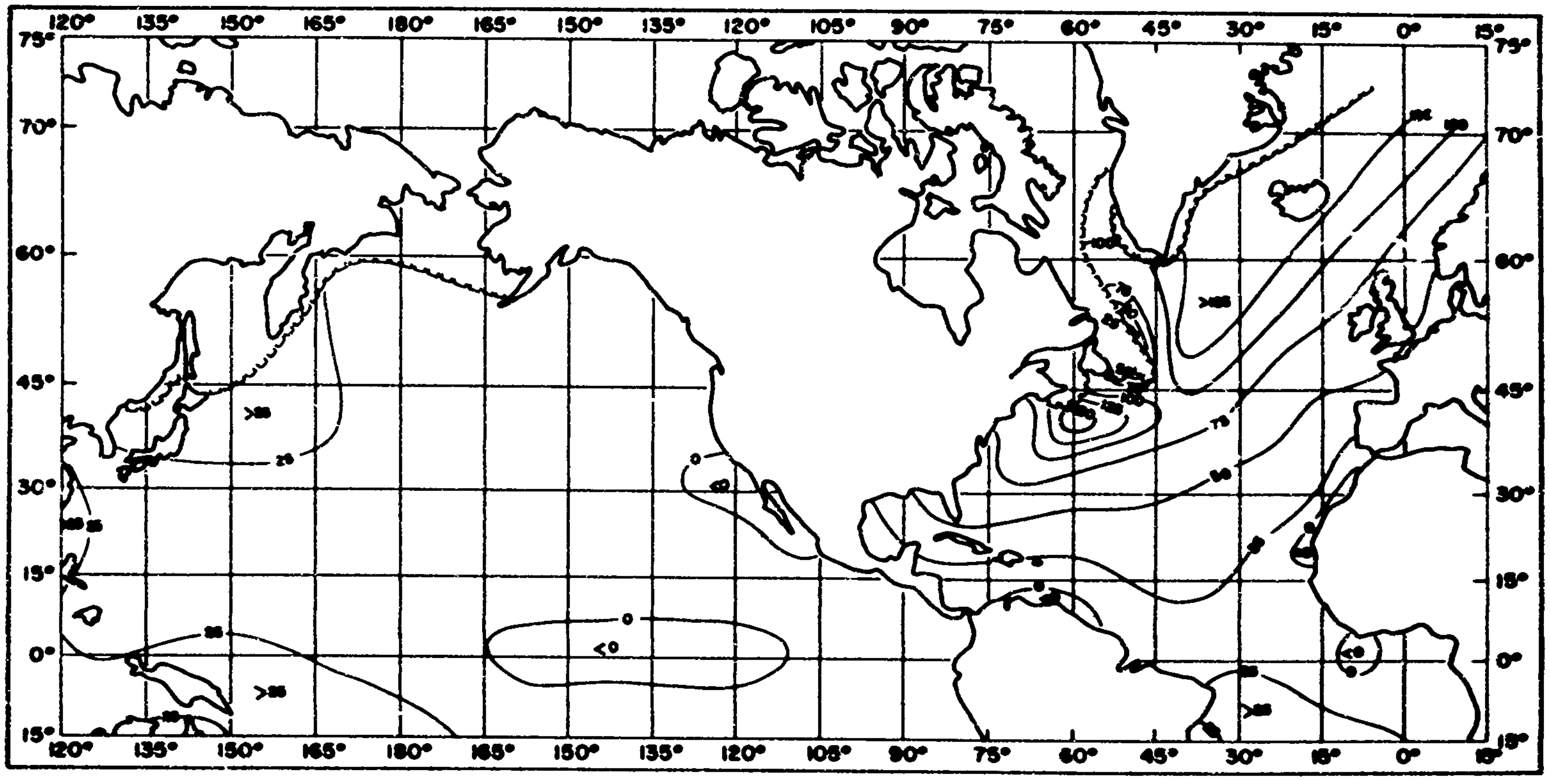

Fig. 6. Sensible heat exchange for the same region in the same units as Fig. 5. After Budyko [1956].

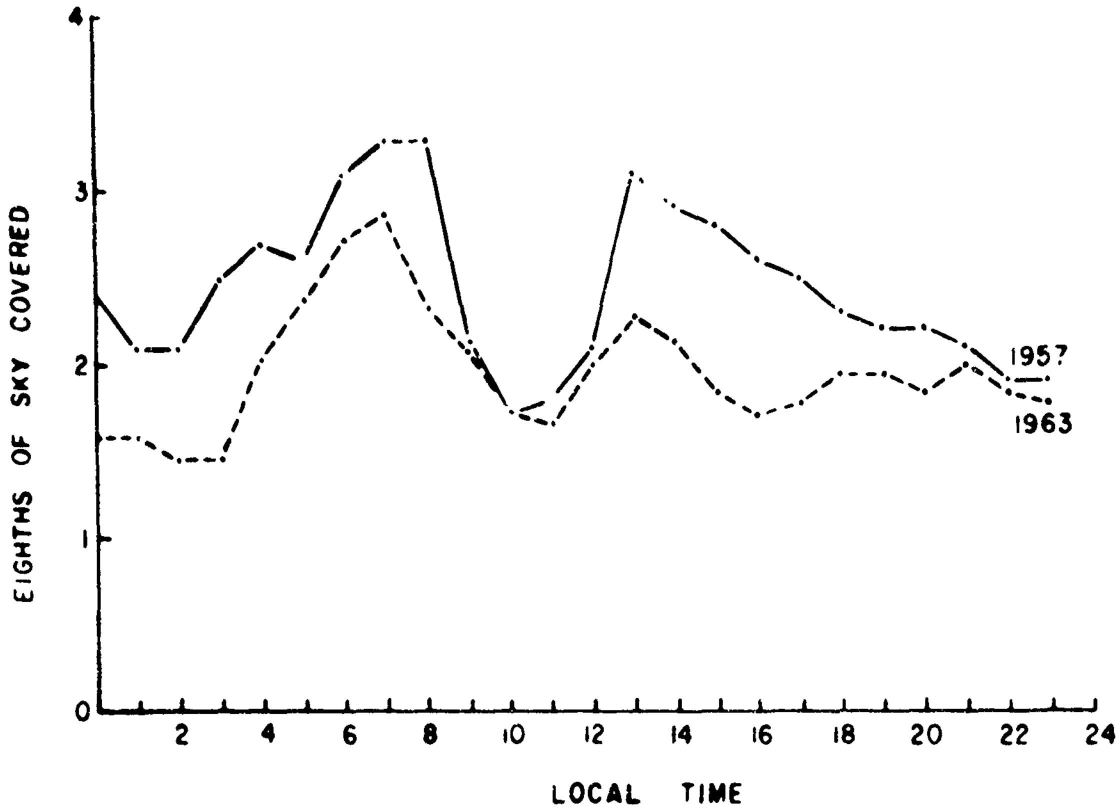

FIG. 7. Diurnal march of low cloud cover (cumulus) in oktas of sky covered during the 1957 and 1963 cruises.

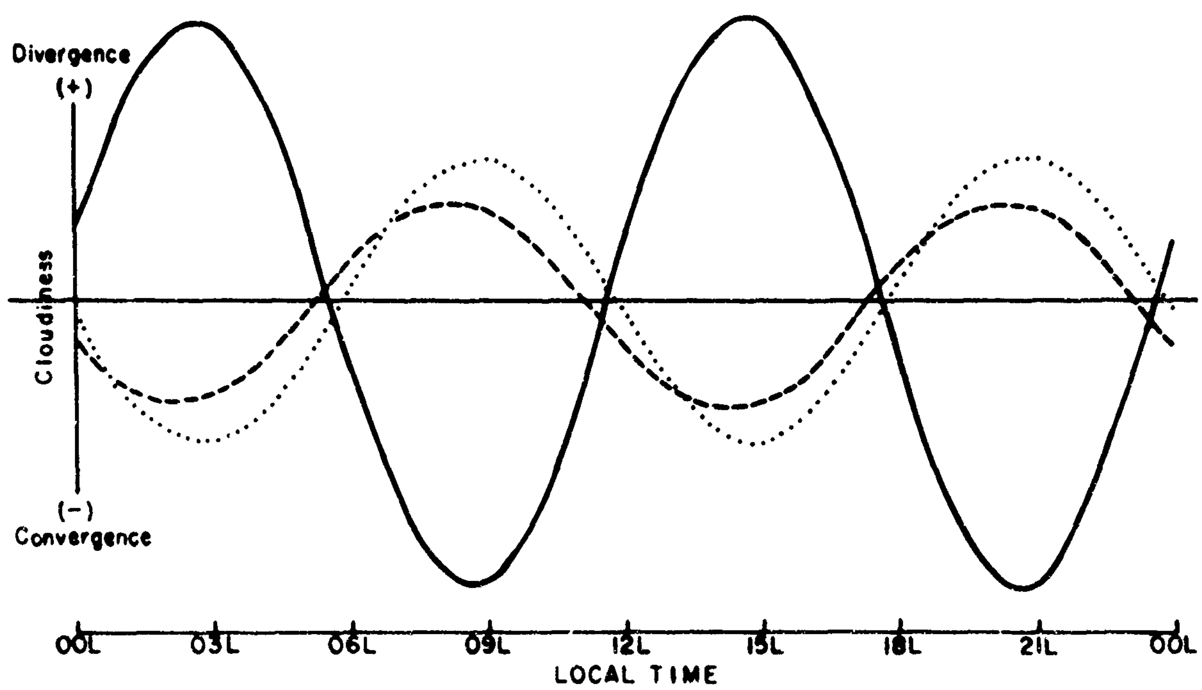

FIG. 8. Combined data related to the divergence-convergence cycle of the atmospheric tide at 3 km. The divergence is indicated by a solid line, the combined 1957 and 1963 ship data (912 observations) by a dashed line and data representing undisturbed days only (714 observations) by a dotted line. The amplitudes are not representative, but phase relationships are shown with respect to local time. After Shibata (1964).

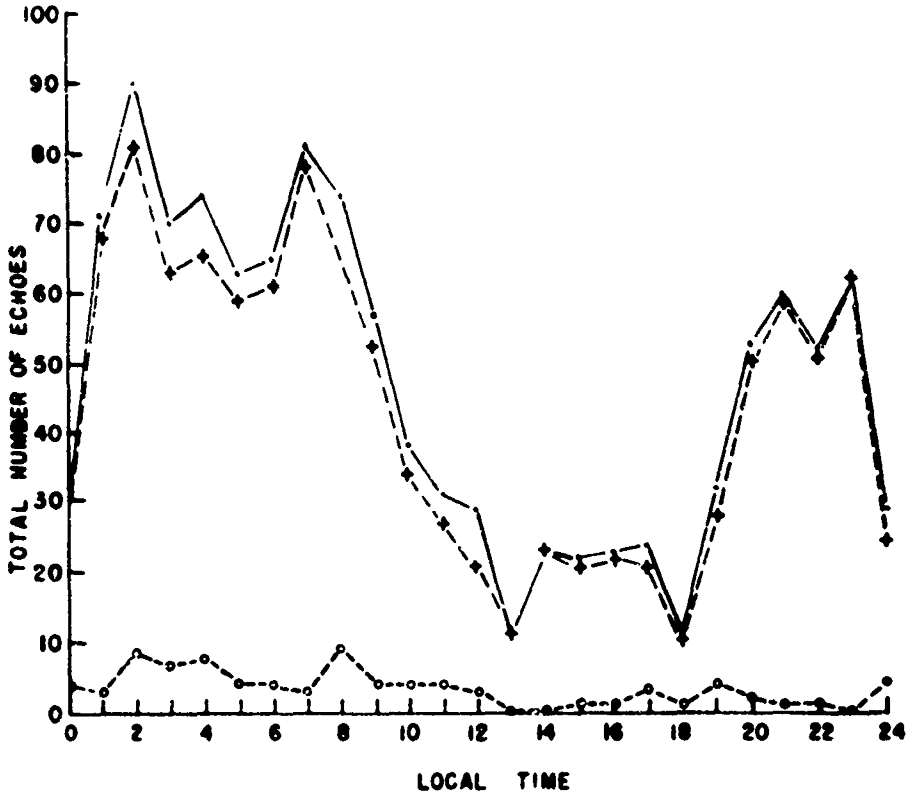

FIG. 9. Precipitation echo frequency as recorded hourly during the 1963 cruise on a 3 cm Decca marine radar with a range of 48 nautical miles. The echoes are classified according to size and intensity into total echo distribution (solid), weak echoes only (dashed) and moderate and strong echoes (dotted).

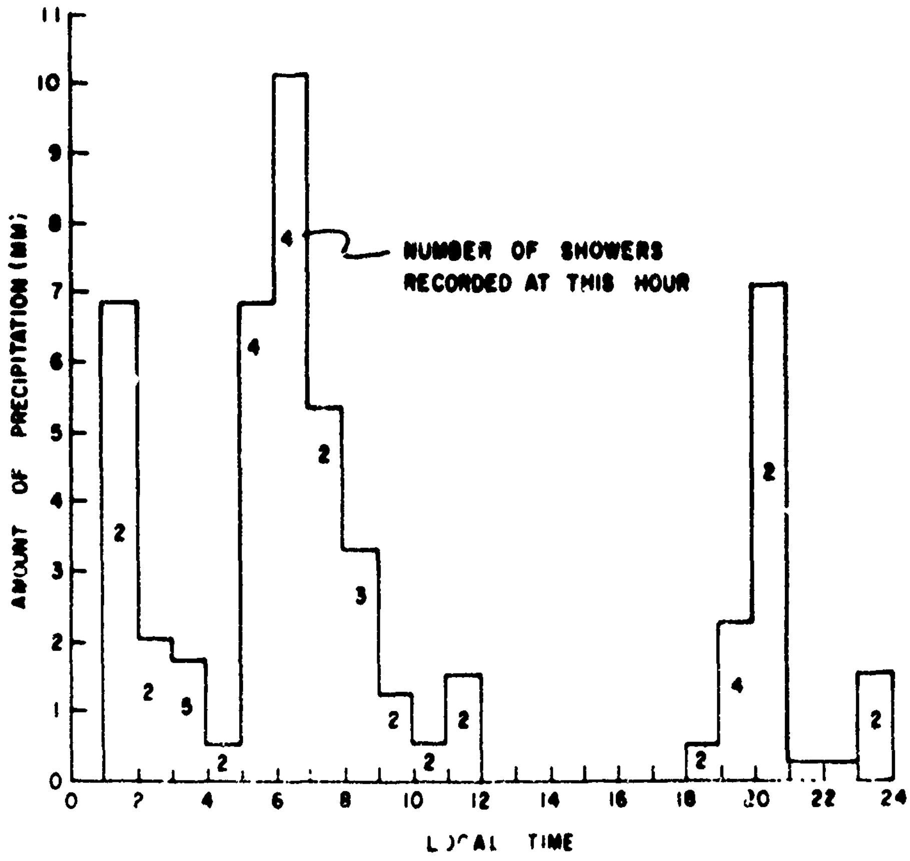

FIG. 10. Diurnal march of precipitation measured on the ship stationed at 13°N 55°W from August 13 to September 3, 1963.

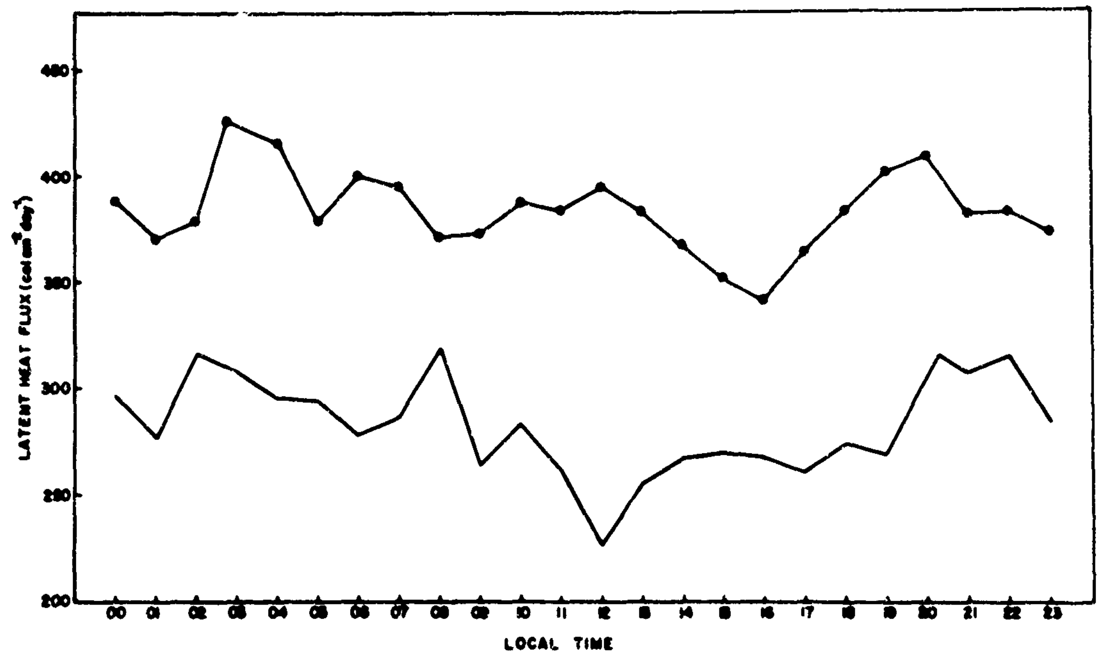

Fig. 11. Diurnal march of latent heat transport (cal cm^{-2} day^{-1}) between the ocean and atmosphere for the two periods of twenty-three days each that the ship was located near 11°N 52°W and 13°N 55°W in 1957 (——) and 1963 (-●-).

DISCUSSION ON GARSTANG'S PAPER

HUBERT: I don't know who here remembers the results I reported last year. This had to do with the different type of cellular convection that one gets over the oceans as a function of whether there is more instability from sensible heat transfer at the low levels or evaporational cooling at the top. At the scale that we can detect on satellite pictures, which means cellular convection of maybe a wavelength of 20 miles, something like that, I think there is very good evidence that the sensible heat can control the type of cellular convection on this scale and the cloudiness that Dr. Garstang shows, which decreases as the sinsible heat transfer decreases, is in the sense that would agree with this. I was suggesting as pure speculation that perhaps as well as the diurnal wave of convergence-divergence that was noticed in the windfield, I believe that this change of relative stability with height might also be a factor. I am not going to outline the whole mechanism now that I went into last year, but perhaps we could get together and discuss this later. It seems that it agrees in the sense, now whether the scale and the quantities involved are large enough, I don't know. I would just like to suggest this for speculation, at least.

KRAUS: You defined the Richardson number in terms of the stability, in terms of the temperature difference alone. Actually in the latitude where you are working, the humidity difference between the sea surface and the layer higher up, is no longer negligible and if you compute the --(not discernible)-- parameter or the flux Richardson number, you find that the humidity difference is of comparative magnitude. I don't think that this would affect your results, it might make them appear more convincing, but I am sure you are right that the daily variation which you showed, namely the flux from the sea has a minimum during the day and a maximum during the night. I am not sure whether it is justified at this stage to relate this to cloudiness and precipitation because that variation can also be interpreted, of course, in terms of a change in radiation, in heating, in cooling, and at the surface of the inversion or at the level of your cloud surfaces; and you find a daily variation also in precipitation from stratus clouds at higher levels which might support that. So, there are other interpretations possible.

GARSTANG: Yes, I agree with you. But as you are probably familiar, the Richardson bulk number is derived from the flux-Richardson number, so that here we are really using the profile coefficient as originally defined by Montgomery, and that is an approximation. Also, I certainly agree that the difference in the profile of humidity must be taken into consideration, or water vapor, or whatever you like to call it. Regarding the second part of your comment, again we basically agree with this as we can't really say what the relative role is of radiational cooling and heating at cloud tops and inversion, and the role of sensible heat at the surface. However, in terms of atmospheric tide this seems to be one of the few parameters that has a semi-diurnal characteristic and seems to be one of the few mechanisms that one can call upon to produce a semi-diurnal variation.

FREEMAN: Well, it is reasonable to say that you found your cloudiness at the time you would expect it from the semi-diurnal tide.

GARSTANG: Yes, certainly.

KRAUS: To what extent can cloudiness be influenced by the difficulty of observing clouds during darkness? Could this peak after dawn be affected by that?

GARSTANG: This is simply an observing problem. I think there is a problem here. This was done, and we are aware of it, of course, and we did it as carefully as it can be done when one uses just an observing technique such as that. I think the great confidence that one can draw upon is the distribution of the radar echoes. The diurnal distribution or semi-diurnal distribution of the radar echoes really leads me to believe that this is correct now. The observations were made carefully.

ESTOQUE: I would like to go back to this proposal by Mr. Hubert, of finding out whether the flux of sensible heat will affect the stability so that you will have the cloudiness or precipitation. This is one of the things that we looked at as a possible explanation for the semi-diurnal variation. We looked at nighttime soundings and daytime soundings, and did not find any significant difference in the stability of the atmosphere.

GARSTANG: I am not surprised at that. Do you want to answer that?

HUBERT: I think Dr. Estoque may have misunderstood. There should not be any difference in the detectable stability, really. We can get together and talk about this afterward, but essentially it is the variation of eddy turbulence through the cloud layer. Naturally, when you have more sensible heat transfer from below, then you will have closer to adiabatic conditions, but at least more eddy turbulence at the lower part than perhaps at the top part of the cloud layer, and I would be surprised, too, if you could find any detectable difference in the stability as seen from the sounding.

RAINFALL FREQUENCY OF THE AREA 0-30°N, 50-100°W

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ABSTRACT

A climatological study of the water area which separates the isolated areas of Middle America is begun using precipitation reports by ships. Although the number and quality of the data leave much to be desired, a number of the resulting patterns of rainfall frequency are interpreted and some temporal and spatial variations are discussed. The equatorial rainfall belt is found to move less than it was believed.

1. INTRODUCTION

The climatology of Middle America is based on relatively good data from isolated areas which are separated by more or less large expanses of water. As most of the areas have a rugged terrain, the climatological evidence changes considerably in short distances, and the question arises: What is due to the general circulation, and what is due to local modification? An approach to answer this question can come only from observations from ocean areas, such as ship, aircraft, and satellite observations.

2. DATA

Several years ago counts of precipitation reports in ship observations were made at the U. S. Weather Records Center in Asheville. These counts have been tabulated for 2x2 degree fields and for months, and they contain the number of rainfall reports as a percentage of all used reports, the absolute number of which is also given.

The frequency tabulations have a series of definite shortcomings, some of which have to be mentioned.

(1) It is not known what data source has actually been used.
(2) It is not known what kind of report has been considered to be a precipitation report.
(3) Precipitation records for typical land areas indicate the existence of position errors.
(4) Snowfall reports near the equator indicate misinterpretations of original observations.
(5) The observation density varies from less than 10 to more than 3000 observations in a given 2x2 degree field for a given month.

In spite of these obvious deficiencies in the material there is some essential substance to it. This becomes evident through the consistency from place to place, and from month to month, and enables us to see the gross climatic features of the region after smoothing the data. The finer details remain questionable and cannot be discussed at this time.

*(Author unable to attend Conference, paper presented by title only, therefore, no discussion follows.)

3. ANNUAL FREQUENCY

Results. In the pattern of annual rainfall frequency shown in Figure 1 are some well defined extremes. As expected, we find a minimum along the coast of Venezuela and northern Colombia. Another minimum lies west and north of Yucatan. This minimum is so consistent that the reduction of cloudiness in that area is a significant feature often noticed in TIROS photographs. As Trewartha (1961) has shown, the decrease of raininess from the southeast to the northwest characterizes also the climate of the Yucatan peninsula.

Maxima of rainfall frequency are found around and east of the Lesser Antilles, in the Gulf of the Mosquitoes, and along 5°N in the Pacific. The last section fits well with the known excessive rainfall at the Colombian coast. Andagoya (5°N 77°W, 250 ft. high) reports 7000 mm (281 inches) as mean annual rainfall and it can be safely assumed that at the seaward slope of the mountains the total is much higher. Although no data on the rainfall frequency of Andagoya are available to the author, the relatively small annual rainfall variation suggests that the frequency is higher than at other stations with comparable amounts. Under this condition the reported range in annual rainfall frequency of 20 percent to more than 30 percent over the sea appears conceivable.

Before studying the seasonal variations as a function of location we consider two different kinds of local changes.

The first kind is characterized by a continuous shift of rainfall conditions within a regime. Cuba, e.g., has in its eastern part a rainfall maximum in May, and in its central and western parts a maximum in June. The transition zone between both regimes shows the maximum at the turn from May to June.

The second kind of local changes is abrupt or overlapping. Puerto Rico, e.g., has its rainfall minimum in January located in a well defined area in the south; in all other portions of the island the minimum is in March. The transition zone between both regimes does not have its minimum in February (which would suggest a continuous transition), but it has two minima, one in January and one in March, with a small secondary maximum in February. This second kind of transition poses the problem of how to present the conditions to the reader while the first kind allows the very simple and illustrative method of charting isochrones.

Both kinds of local changes of rainfall regimes exist also over water areas. But while the January minimum in Puerto Rico can be explained as a chinook (foehn) effect of increased wind effects from the north ("northers"), such a simple explanation is generally still lacking for overlapping rainfall regimes over sea.

Though most features of the rainfall patterns cannot as yet be explained, the total data coverage of the region permits to observe the data representing the land stations as a part of the general pattern. In particular, the reason for the complexity of the annual rainfall variation of the Florida peninsula becomes obvious. The rainfall regimes of the Gulf of Mexico and of the Atlantic are substantially different in some months, and Florida is influenced by both of them. Figure 2 presents the annual variation of rainfall frequencies for a field east and a field west of Florida. These patterns go right to the coasts, and there is no indication that there is an area with an April minimum between the May minimum of the Gulf and the March minimum of the Atlantic.

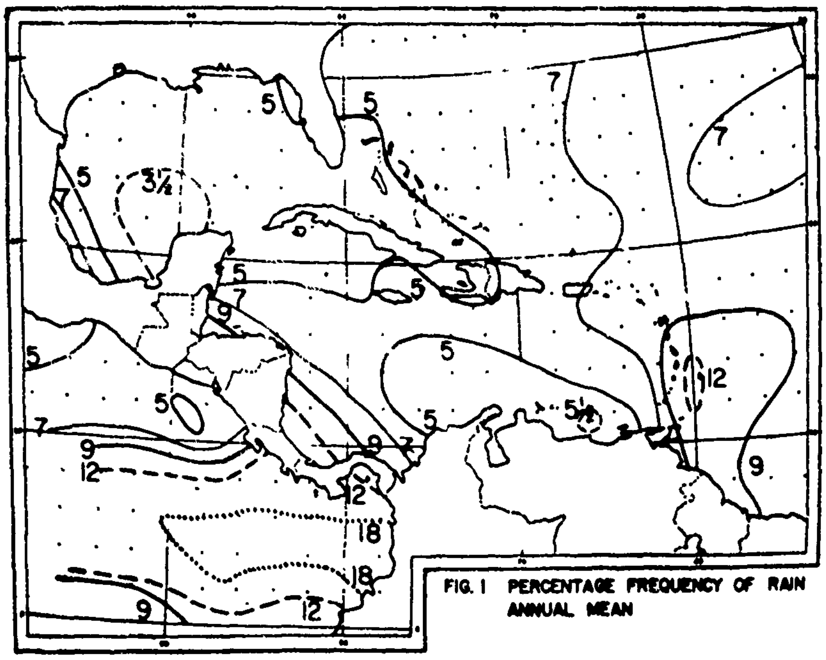


FIG. 1 PERCENTAGE FREQUENCY OF RAIN
ANNUAL MEAN

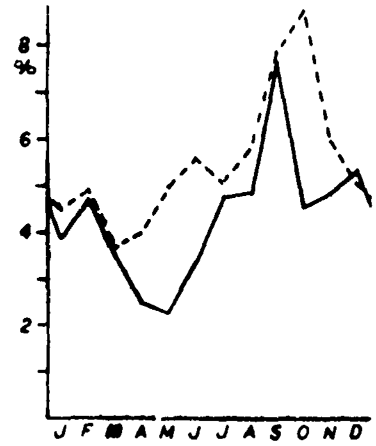


FIG. 2

ANNUAL VARIATION OF RAINFALL FREQUENCY

——— field 26-28°N 88-90°W
-------- field 26-28°N 74-76°W

(Ordinate scale: number of precipitation reports as percentage of the total number of observations)

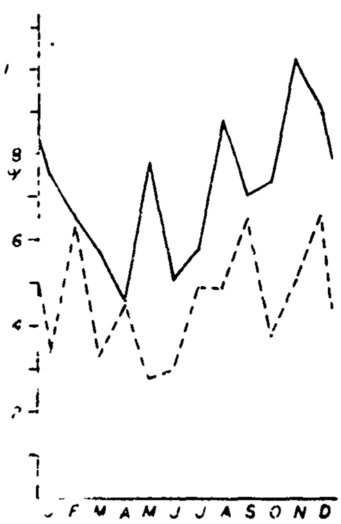


FIG 3

ANNUAL VARIATION OF RAINFALL FREQUENCY

——— field 20-22°N 56-58°W
········ field 28-30°N 90-92°W

(Ordinate scale number of precipitation reports as percentage of the total number of observations)

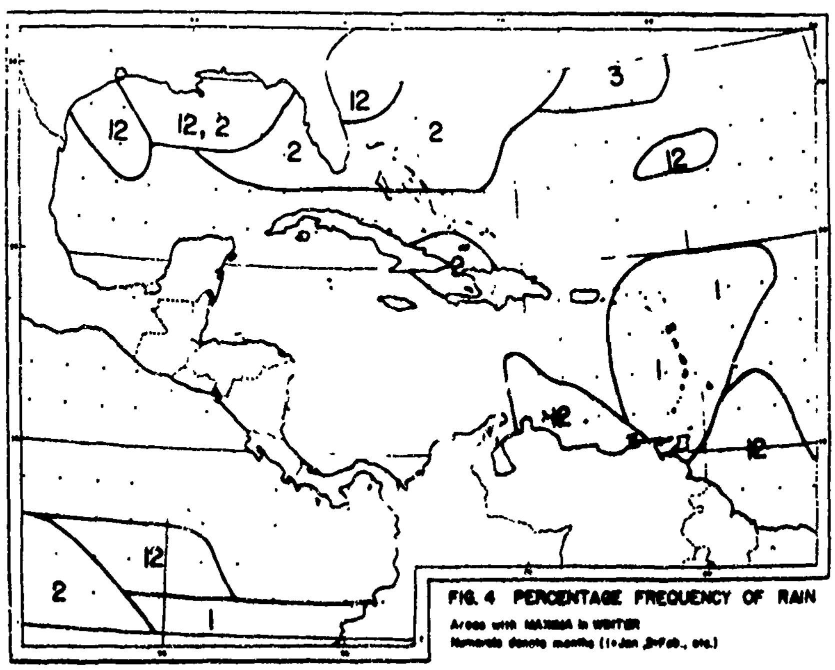


FIG. 4 PERCENTAGE FREQUENCY OF RAIN
Areas with MAXIMA in WINTER
Numerals denote months (1=Jan, 2=Feb., etc.)

4. SEASONAL FREQUENCIES

The number of annual rainfall maxima varies in the region. There are some small areas with only one rainfall maximum, two of which should be mentioned. One comprises most of the southern part of the Gulf of Mexico and the other consists of the strip 6-8°N 82-90°W in the Pacific. Both areas have their highest rainfall frequency occurring in September. The minimum occurs in February (or March) in the Pacific area, and in May (or April) in the Gulf. In this respect they are equal to their surroundings.

Approximately one-half of the region has two rainfall maxima in summer, a broad minimum in winter, and a secondary minimum in mid-summer. This type of rainfall regime is found from south of Central America across most of the isthmus to Cuba and extending from there eastward up to at least 50°W staying south of 25°N. The double peak regime also dominates the open Caribbean Sea, but not the islands from the eastern tip of Cuba eastward until Martinique.

When the minimum and the maximum are very widely separated, a third maximum may be inserted. The solid line in Figure 3 gives an example for this as it is found in the Atlantic halfway between the Lesser Antilles and the Bermuda Islands. All three maxima are very prominent and can be followed over considerable areas.

5. WINTER RAINS

Wide areas of our region have rainfall maxima in winter. One can distinguish between maxima that are belated fall maxima and those that are genuinely winter maxima. In the first of these types the isochrones of the rainfall maximum proceed gradually from earlier occurrences in the surroundings to the late maximum at the location under consideration. In the second type there is no such transition.

The belated maxima will be discussed with the summer rains.

The independent winter rain maximum can be found almost always north of 25°N and is typical for land stations near the northern part of the Gulf. In many places the annual variation of rainfall frequency has two peaks in winter (such as in the dashed diagram of Figure 3) because "northers" reduce the moisture drastically in mid-winter (January and partly February) (see Figure 4).

Another winter rain maximum which is not a belated fall maximum exists in the Pacific Ocean southwest of Panama. It emerges in November at 5°N 85°W and is separated from the September maximum. This is one of the two areas without a maximum in late spring or early summer. (The other such area covers most of the Gulf of Mexico, see Figure 6, and the solid curve of Figure 2). From there the rainfall maximum moves to the south and to the southwest.

6. THE EQUATORIAL RAINFALL BELT

Figure 5 is composed of time cross sections along the 81st, 85th, and 93rd meridians from the equator northward over sea. The dashed lines indicate temporal rainfall minima, and the temporal maxima lie somewhere between them. It is obvious that the 81st (bold lines) and the 85th (medium lines) meridians have two maxima and minima each, while the 93rd (thin lines) has three of them south of 6°N. The positions of the extremes do not match the concept of a migrating ITC. If the ITC were migrating, the dashed lines in the upper part of the figure would slope downward to the right. In other words, the spring extremes would occur later in the higher latitudes than in the lower latitudes. In the lower part of Figure 5, they would run downward to the left (i.e., the fall extremes would occur earlier at higher than at lower latitudes). This is the case only at 85°W and 93°W. But a perusal of the monthly maps (which are not shown here) discloses that the minima are local and have nothing to do with a meridional movement of the rainfall belt.

The annual variation of the position of the area of highest rainfall becomes evident from the solid lines of Figure 5. The most conspicuous (and for many meteorologists, unexpected) fact is the very slight movement of the rainfall belt. It is

4 deg. latitude (from 1.5 to 5.5°N) at 81°W
4.6 deg. latitude (from 1.8 to 6.4°N) at 85°W, and
4 deg. latitude (from 3 to 7°N) at 93°W,

and it stays far south of Central America always at any meridian. The rainfall frequencies within the belt are very high, more than 30 percent in summer, and 6 percent in March (81 and 85°W) or April (93°W). The secondary minimum is weak and amounts to

25 percent in August at 81°W
26 percent in October at 85°W
15 percent in July at 93°W

The rainfall frequency drops sharply at relatively short distances north and south of the belt. This effect is, of course, at its extreme when Central America is already in its dry season, and the rains are still strong over the Pacific, as in December. At that time we find a drop from 30 percent at 5°N to 5 percent at 9°N, and to 1 percent at 13°N when we move northward along 91°W.

7. EARLY SUMMER RAINS

Most of the region has two rainfall peaks in summer and fall. Isochrones of their occurrence can be charted and are shown in Figures 6 and 7. The first maximum begins in May (if we neglect the rather complex conditions near 30°N 50°W where there is a maximum in March and April) in most of the Atlantic part of our region, in the central Caribbean, and in the western part of the equatorial rainfall belt. The solid line of Figure 3 shows that the onset of the rainy season can be quite sudden, and that it can be followed closely by a marked decrease of raininess. But there can be a gradual emergence of the rainy season such as there is south of Puerto Rico where we find the following rainfall frequencies:

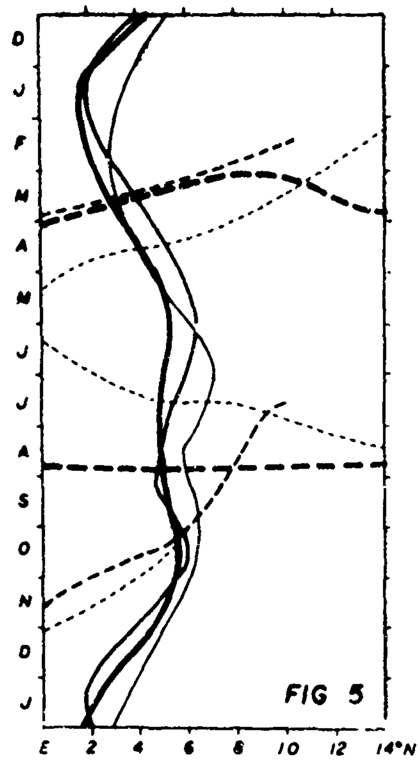
TABLE I

Rainfall Frequencies South of Puerto Rico

Field	J	F	M	A	M	J	J	A	S	O	N	D
16-18°N 66-68°W	7.4	6.0	4.0	4.7	5.6	6.5	7.2	7.9	7.9	9.2	9.3	7.8%

Almost the entire remainder of the region has its early summer maximum occur in June.

Most of the few spaces left have their first summer maximum in July. But there are also areas which had their first maximum in May and have their second in July. This area moves southward in August where it overlaps with areas that had their first maximum in May or June, and with a small area



TIME CROSS SECTIONS OF RAINFALL MAXIMUM
(solid lines) AND MINIMA (dashed lines)

bold lines - 81°W (south of central Panama)
medium lines 85°W (south of Lake Nicaragua)
thin lines - 93°W (south of southeast Mexico)

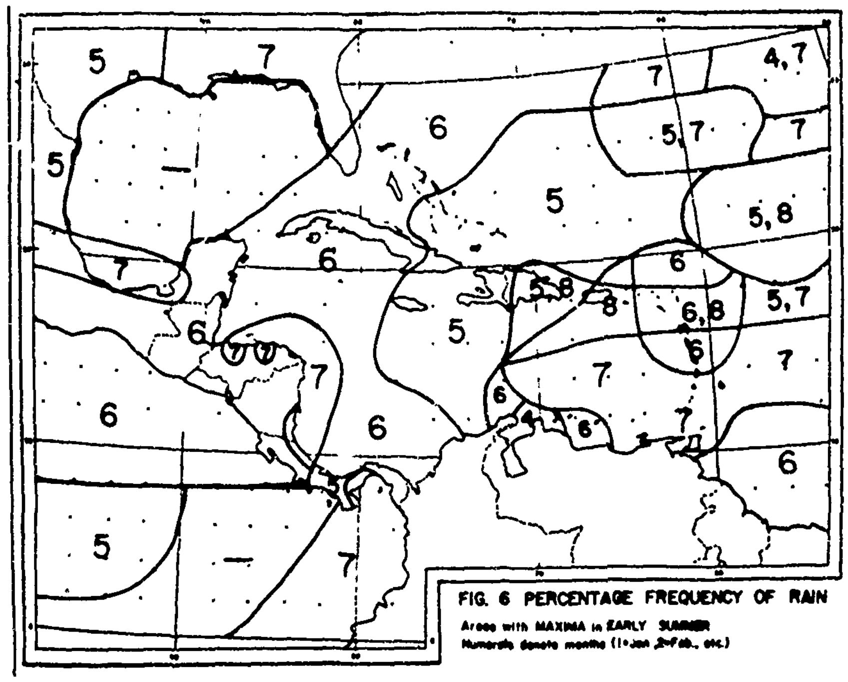


FIG. 6 PERCENTAGE FREQUENCY OF RAIN

Areas with MAXIMA in EARLY SUMMER
Numerals denote months (1=Jan, 2=Feb., etc.)

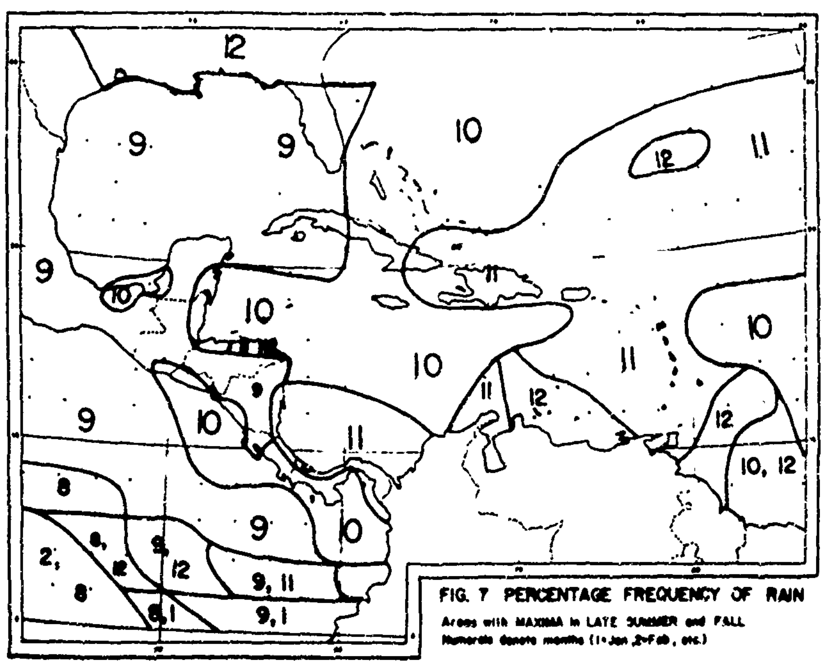


FIG. 7 PERCENTAGE FREQUENCY OF RAIN

Areas with MAXIMA in LATE SUMMER and FALL
Numerals denote months (1=Jan, 2=Feb., etc.)

where August displays a very weak first maximum.

There is no simple scheme that describes the time pattern of the first summer rain maximum. Two tendencies can be discerned. In the southwest and in the southeast corners of our region we find the south-north progressions May-June-July along the 93rd meridian, and June-July-August along the 55th meridian as well as along the 67th. The other tendency is a radial spreading from the area north of Hispaniola. Among the details only two should be mentioned. First, along the line from (approximately) 22°N 72°W to 2°N 82°W, the rains spread southward. Second, the July rains of the southern part of the Gulf of Mexico may be considered as the end link of a May-June-July chain progressing northward as well as such a chain progressing westward.

The northward directed "chain" of the previous paragraph should not be confused with the northward movement of the rainfall belt. The following table makes this difference clear.

TABLE II

Rainfall Frequencies Along 93°W for Selected Months

	April	May	June	July	August
18-20°N	2.5	2.0	3.2	3.5/	3.0
16-18°N			over land		
14-16°N	1.7(?)	6.0	15.4/	11.2	7.8
12-14°N	1.0	7.0	13.5/	11.2	8.0
10-12°N	1.3	10.2	14.3/	12.7	10.0
8-10°N	2.5	15.4	16.5/	13.0	19.0
6- 8°N	3.3	18.6/	17.3	15.3	23.6
4- 6°N	6.5	21.6/	20.0	14.0	24.8
2- 4°N	5.8	13.6/	11.9	10.1	17.1
0- 2°N	1.6	1.5	0	7.5	7.7

——— = rainfall belt; / = local rainfall maximum

8. LATE SUMMER AND FALL RAINS

The pattern of this rainfall season (Figure 7) can be described in simple terms. The rains begin in the southwest corner of our region in August and spread from there to the north and to the northeast arriving five months later (in December) in the open Atlantic and at the north and northeast coasts of South America.

9. FREQUENCY MINIMA

The main rainfall minimum lies in the entire region in the first half of the year. The area where it first occurs is the Pacific side of Central America and the adjacent waters.

Further details can be gleaned from Figure 8. In the northeast of the region there are two minima in the first half of the year: in March and in June. In some part of that area both are secondary minima and the main minimum occurs in August.

The Gulf of Mexico has to be mentioned because its minimum in May has neither continuous transitions to the minimum of Central America nor to that of the Florida Straits. The Gulf coast of Texas has even a maximum at the same time (compare Figures 6 and 8).

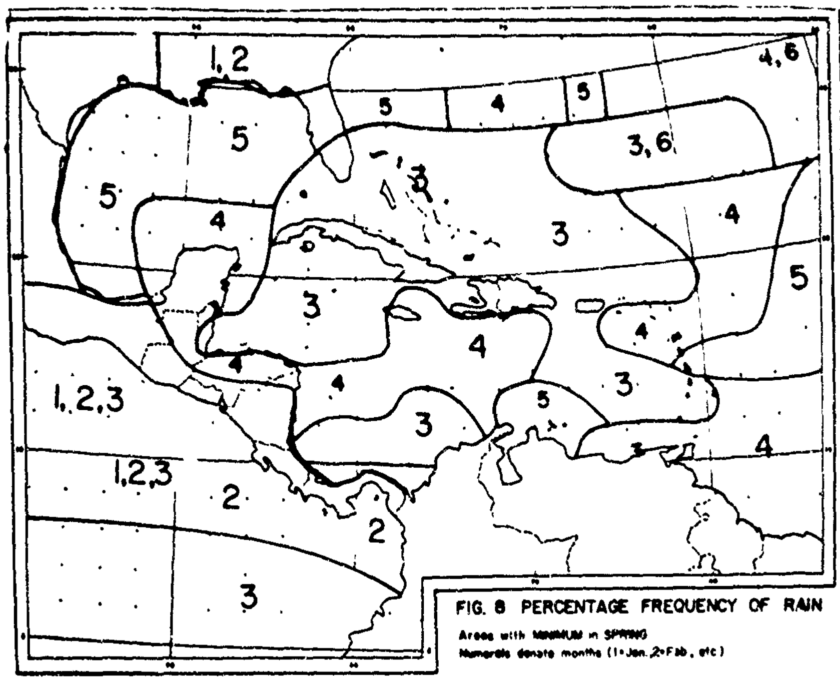


FIG. 8 PERCENTAGE FREQUENCY OF RAIN

10. DETAILS OF LOCAL CHARACTER

The "northers" of winter produce sharp local rainfall maxima at the north coast of Honduras in the period November through February and at the north coast of Jamaica in the period January through March, whereas the north coasts of Cuba and Hispaniola are effected to a lesser degree.

In most months the rainfall frequency decreases sharply downstream west of the Lesser Antilles. This effect is most pronounced in February, March, June, and July when the frequency drops to half its value at the Antilles.

In certain months there is a meridian of maximum rainfall frequency over the open ocean between 24 and 30°N, viz.

62°W in May
68°W in June
63°W in July
approx. 68°W in August (ill defined)
66°W in September.

Perhaps this is an effect of a double cell system at the horse latitudes.

The Caribbean has generally a smaller percentage of rainfall observations than the Atlantic. But in September through January the frequencies become equal or even reversed. This is due to the stronger seasonal changes in the Caribbean Sea.

A repetition of this study with additional properly checked data might alleviate some of the interpretation problems that evolve due to ill defined maxima and minima. Many additional interesting details might be revealed in such a study. It is hoped that studies such as this will aid in a better understanding of the general circulation as it relates to the tropics.

REFERENCE

Trewartha, G. T., 1961, The Earth's Problem Climates, Univ. of Wisconsin, Madison, Wisconsin.

SESSION SUMMARY AND QUESTIONS BY JOHN FREEMAN

FREEMAN: It was suggested that we make certain about further research and suggestions about tropical meteorology and I have been able to remember four such things right now. There is a need to train observers and an implication of other meteorologists and have them available in certain countries by whatever political and tangible means that can be done. That was one comment that came up. There should be some effort to give equipment to countries in tropical regions who have trained observing staffs. The last question was posed by Dr. Estoque, I think. Will a flux of sensible heat affect cloudiness and precipitation over the ocean? Was that in the form of a question, or did you then answer the question, Dr. Estoque?

ESTOQUE: I think this was more of a comment by Dr. Kraus. It was also implicit in the stability question.

FREEMAN: You would say there was no work called for then?

ESTOQUE: Yes, I would think there should be some work.

FREEMAN: What should this work be?

ESTOQUE: Well, I would suggest first an observational program to determine the correlation between the stability changes which are associated with these sensible heat flux variations during the daytime and then having described this, we might try to formulate this problem quantitatively and try to verify the observations.

FREEMAN: Then I remember from Dr. La Seur's paper that he promised he would re-stratify his data, feeling that would make a better presentation of it, and would give some attention to presentation in the form of frequency rather than amount. I would like to go through the speakers here and ask them for comments of this nature. That is, did you leave us with a question that I did not detect, or did you outline your plan for next year in some summarized form? I will go through the scheduled speakers first. Dr. La Seur.

LA SEUR: My answer to that would take much too long, John. I happen to have a 30 page proposal that I wish to discuss with representatives of the Army to continue our program and do that in that manner.

FRISBY: One of the crying needs for this section of the program is to get more information on cloud-top heights.

FREEMAN: Now, I would like to ask you a question. What is the lowest latitude at which you have found hail?

FRISBY: Oh, about 25 degrees. I would like the data between 20 degrees and the equator.

GARSTANG: I think as perhaps Dr. Kraus would agree with me, the whole approach that was used is far too suspect in many instances to really say that this is the only way one should do it. It involves an approach both experimental and theoretical to really iron this question out. There is no question of transfer of latent sensible heat and momentum.

FREEMAN: Could you summarize the kind of measurement you think is needed, or is this another 30 page proposal?

GARSTANG: Again I think it gets into far too involved discussion to mention at this point.

FRISBY: I just want to qualify my statement. The latitude is right, but I'm particularly interested in cloud-top heights at times of maximum convection.

FREEMAN: A radar observation should do you some good, RHI observations from a radar, say. Dr. Lessman, would you have anything to add in the nature of work you feel is suggested by what you presented? This includes your work showing the precipitation, and also I would say your statements about your experiences and your problems in El Salvador. Would you have anything that you feel would be worth consideration during the next year?

LESSMAN: Well, I would like to underline that it proved very fruitful when investigators came to our countries and not only asked for material, but we the local tropical meteorologists had the opportunity to inform them about our experiences. Furthermore, I would like to say that I think it is very important to do something to improve the quality of the observations, and as was just mentioned to train observers and also inspect the observing stations. I know that in Central America, there are weather services which do not inspect the stations they have, so they do not know what they are receiving.

FREEMAN: Now, I am sure that this could go on all night, but if there is someone other than the speakers who feels there is a point that must be added, would you please bring it forward at this time.

GERRISH: I can't resist the temptation to add something to the great emphasis that is being placed on quality of data, and quantity, as we all recognize it. This is not the complete answer. Mr. Dunn, I think, installed 13 precision aneroid barometers in the Bahamian network to improve the quality of observations there. These instruments require an observer with zero training to read the dial. If you can read a watch, you can read an aneroid barometer, yet there appears to be some reluctance to read these instruments accurately. I'm afraid that if you were to inspect some people every week, they would still resist reading the instruments correctly in that there is a nationalistic problem or political problem, or problems of this sort that one has to contend with, and probably always will have to contend with as far as data in the tropics are concerned. This is unfortunate. You can get the best equipment, you can give them the best training and there will still be occasions somewhere, here and there, that they will be reluctant to provide accurate observations for various reasons. Certainly we need more data and better equipment, but we need cooperation also.

FREEMAN: Perhaps some of the people in Barbados could be utilized in this respect. I think that would make it a little bit easier and overcome some of this national feeling, and so this may be something that we could take as an unofficial crusade; that is to begin to get these people who are fairly well educated and reasonably moral from the standpoint of taking observations, to try to get them in as many places as possible in this area.

A SYSTEM OF TWO-DIMENSIONAL MESOSCALE ANALYSIS AND FORECASTING WITH FRICTION AND MOISTURE

John C. Freeman, Jr., and Joseph L. Goldman
National Engineering Science Company

ABSTRACT

The effects of friction and moisture are added to a two-dimensional model of tropical flow associated with the trade-wind inversion under balanced and unbalanced conditions. A model containing some of the recently found characteristics of waves in the trade-wind inversion is developed and a simplified approach to the dynamic solution of the problem of motion of this wave is presented.

1. FLOW WITH FRICTION

The system of analysis outlined here is based on a theoretical development that involves the properties of an inversion. The trade-wind inversion is indigenous to the tropics during much of the year and in many cases the actual forecast method may follow the inversion. However, since we want to decrease the scale of the analysis and prediction to a size smaller than any known radiosonde network, we present a method that does not require measurement of the inversion even though the measurements can be used.

In the development below, H is the height of an upper level pressure surface, and h is the height of an inversion surface. The quantity is a "buoyant gravity", that is, in all respects treated like a reduced gravity acting on the inversion surface as if it were a water surface.

The following symbols have their traditional meaning in theoretical meteorology:

u = west component of the wind in lower layer
v = south component of the wind in lower layer
g = acceleration of gravity
u' = west component in upper layer
v' = south component in upper layer
θ = potential temperature at inversion in lower layer
θ' = potential temperature at inversion in upper layer

We will assume that u and v are the values above the friction layer and that they are reduced by friction by an amount $-F\mathbf{v}|\mathbf{V}|$ where $\mathbf{V} = (u, v)$ the wind vector. There is also an effect of the friction term on the height which is a result of the turning of the winds in the friction layer. This contribution to $\partial h/\partial t$ (even when the upper wind divergence is zero) is given by

$$K\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

where K is the friction factor.

With these modifications and with the assumption that u and v are not functions of height, we can write the equations of motion and continuity for this problem:

$$\frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + v\frac{\partial u}{\partial y} = -g\frac{\partial H}{\partial x} - \gamma\frac{\partial h}{\partial x} + fv - Fu\sqrt{u^2+v^2}$$

$$\frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} = -g\frac{\partial H}{\partial y} - \gamma\frac{\partial h}{\partial y} - fu - Fv\sqrt{u^2+v^2}$$

$$\frac{\partial h}{\partial t} = -\frac{\partial}{\partial x}hu - \frac{\partial}{\partial y}hv + K\left(\frac{\partial}{\partial x}v\sqrt{u^2+v^2} - \frac{\partial}{\partial y}u\sqrt{u^2+v^2}\right)$$

This is a complete set of equations for u, v, and h provided we are given H. We assume that H(x, y, t) is given by a numerical weather prediction scheme that is either incorporated into this problem as a macro-scale forecast or is sent in from a headquarters forecasting unit. We also assume that $fu' = -g\,\partial H/\partial y$ and $fv' = g\,\partial H/\partial x$. The equations then become

$$\frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + v\frac{\partial u}{\partial y} = -\gamma\frac{\partial h}{\partial x} + f(v-v') - Fu\sqrt{u^2+v^2} \qquad (1)$$

$$\frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} = -\gamma\frac{\partial h}{\partial y} - f(u-u') - Fv\sqrt{u^2+v^2} \qquad (2)$$

$$\frac{\partial h}{\partial t} = -\frac{\partial}{\partial x}hu - \frac{\partial}{\partial y}hv + K\left(\frac{\partial}{\partial x}v\sqrt{u^2+v^2} - \frac{\partial}{\partial y}u\sqrt{u^2+v^2}\right) \qquad (3)$$

If the low level is dry or h is less than the convective condensation level everywhere, then $\gamma = \gamma_0$, a constant.

If the low level inversion is above the convective condensation level, then $\gamma = \gamma(h)$*.

We assume a very simple form for $\gamma(h)$. Namely,

$$\gamma(h) = \gamma_0 - \frac{\gamma_0(h-h_0)}{(h_B-h_0)}$$

$$= \frac{\gamma_0(h_B - h_0 - h + h_0)}{(h_B-h_0)}$$

$$= \frac{\gamma_0(h_B - h)}{(h_B-h_0)}$$

where h_o is the convective condensation level and h_B is the height at which the values of θ are the same in the two air masses.

Along lines at which h_B is exceeded, there is no resistance to vertical motion and air can cross the line and disappear if it is pushed across. We will give these lines and fronts special treatment.

The first problem to be attacked is the problem with dry air and no fronts.

*There is some evidence that we should distinguish between rising and descending air, but we won't do that in this system.

2. FRICTIONAL MODIFICATION OF GEOSTROPHIC FLOW

The simplest problem that still is two-dimensional and makes sense is the very slow motion of air and inversion so that we can say

$$f(v - v') = +\gamma \frac{\partial h}{\partial x} \qquad f(u - u') = -\gamma \frac{\partial h}{\partial y}$$

In other words, we make the geostrophic approximation throughout. This results in a set of equations:

$$-\gamma \frac{\partial h}{\partial x} + f(v - v') = 0 \tag{1}$$

$$-\gamma \frac{\partial h}{\partial y} - f(u - u') = 0 \tag{2}$$

$$\frac{\partial h}{\partial t} = K\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right) \tag{3}$$

This is a complete set of equations for u, v, and h if we are given u′ and v′.

The principal difficulty with this set of equations as a forecast aid is the lack of information concerning h in most situations. There are some measurement programs that will result in h on a usable scale. However, for ordinary use we need to use some parameter that can be measured at the surface.

If we write

$$\frac{\partial h}{\partial x} = \frac{f}{\gamma}(v - v') \qquad \frac{\partial h}{\partial y} = -\frac{f}{\gamma}(u - u')$$

Expanding Equation (3) and taking partial derivatives with respect to x and y respectively, and neglecting second order terms in u and v, we have

$$\frac{d}{dt}\frac{\partial h}{\partial x} + 2\frac{\partial u}{\partial x}\frac{\partial h}{\partial x} + \frac{\partial v}{\partial x}\frac{\partial h}{\partial y} + \frac{\partial v}{\partial y}\frac{\partial h}{\partial x} = K\frac{\partial}{\partial x}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

$$\frac{d}{dt}\frac{\partial h}{\partial y} + \frac{\partial u}{\partial y}\frac{\partial h}{\partial x} + 2\frac{\partial v}{\partial y}\frac{\partial h}{\partial y} + \frac{\partial u}{\partial x}\frac{\partial h}{\partial y} = K\frac{\partial}{\partial y}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

$$\frac{d}{dt}\frac{\partial h}{\partial x} = -2\frac{\partial u}{\partial x}\frac{\partial h}{\partial x} - \frac{\partial v}{\partial x}\frac{\partial h}{\partial y} - \frac{\partial v}{\partial y}\frac{\partial h}{\partial x} + K\frac{\partial}{\partial x}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

$$\frac{d}{dt}\frac{\partial h}{\partial y} = -\frac{\partial u}{\partial y}\frac{\partial h}{\partial x} - 2\frac{\partial v}{\partial y}\frac{\partial h}{\partial y} - \frac{\partial u}{\partial x}\frac{\partial h}{\partial y} + K\frac{\partial}{\partial y}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

Substituting in above equations, we get

$$\frac{d}{dt}\frac{f}{\gamma}(v-v') = -2\frac{f}{\gamma}\frac{\partial u}{\partial x}(v-v') + \frac{f}{\gamma}\frac{\partial v}{\partial x}(u-u') - \frac{f}{\gamma}\frac{\partial v}{\partial y}(v-v')$$
$$+ K\frac{\partial}{\partial x}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

$$-\frac{d}{dt}\frac{f}{\gamma}(u-u') = -\frac{f}{\gamma}\frac{\partial u}{\partial y}(v-v') + 2\frac{f}{\gamma}\frac{\partial v}{\partial y}(u-u') + \frac{f}{\gamma}\frac{\partial u}{\partial x}(u-u')$$
$$+ K\frac{\partial}{\partial y}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

We see that with this substitution we have two equations in u and v, and h is not needed.

If we know f and γ are constant, we can write

$$\frac{d}{dt}(v-v') = -2\frac{\partial u}{\partial x}(v-v') + \frac{\partial v}{\partial x}(u-u') - \frac{\partial v}{\partial y}(v-v') + \frac{\gamma}{f}K\frac{\partial}{\partial x}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

$$\frac{d}{dt}(u-u') = \frac{\partial u}{\partial y}(v-v') - 2\frac{\partial v}{\partial y}(u-u') - \frac{\partial u}{\partial x}(u-u') - \frac{\gamma}{f}K\frac{\partial}{\partial y}\left(\frac{\partial}{\partial y} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

Noting that this can be written in this way, we do not need to solve this complicated equation. We can form our computation with Equations (1), (2) and (3) taking an arbitrary initial value of h at some point. We compute $\partial h/\partial x$, $\partial h/\partial y$ from Equations (1) and (2) and integrate to find h. Then we compute $\partial h/\partial t$ and find h at a new time.

For the purpose of computation, it is worthwhile to set $P = \frac{\gamma h}{f}$. Then we have

$$\frac{\partial P}{\partial x} = (v-v')$$

$$\frac{\partial P}{\partial y} = -(u-u')$$

$$\frac{dP}{dt} = \frac{\gamma K}{f}\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right)$$

Now, in order to start this computation on any one day, we need to arrive at a value of $\gamma K/f$.

We obtain this by measuring u(x, y) v(x, y) at two given times and comparing it with a forecast and estimating the $\gamma K/f$ required to get the best answer. We use this value for the forecast. If there is an obvious reason for γ, or K to change, (such as a front or a coast line) then we will compute values of $\gamma K/f$ to match the values.

This method is expected to be good for temperature latitudes. It is the method recommended for making quick local forecasts.

Small general purpose machines will likely be used by local weather stations in a very few years to solve equations of similar difficulty. The central computer can hardly be programmed for special local problems and it is unlikely that the input data can be made available to a forecast center. For example, at the present time it is unlikely that a numerical prediction of local frontal motions is made anywhere.

If we apply the consideration of moisture to this particular problem, we can get a problem that makes some sense and perhaps will be useful. By simply noting that we can form a function Q such that $\frac{\partial Q}{\partial x} = \gamma(h) \frac{\partial h}{\partial x}$ etc., or Q(h) such that $\gamma = \gamma(Q)$, we will have

$$\frac{\partial Q}{\partial x} = (v - v')$$

$$\frac{\partial Q}{\partial y} = -(u - u')$$

$$\frac{dQ}{dt} = J(Q)\left(\frac{\partial}{\partial x} v\sqrt{u^2+v^2} - \frac{\partial}{\partial y} u\sqrt{u^2+v^2}\right.$$

The determination of Q and J(Q) is a similar but more complicated problem than finding $\gamma K/f$. However, this can be determined from local and recent data and then used in the forecast. This system of prediction takes friction and moist air into account and can be expected to be of use in local forecasting.

This type of analysis and prediction lends itself to the study of fronts. The warm air is computed to move over the cold air with the friction factor K (probably) reduced. u and v are computed in the warm air. The values u and v, for the warm air over the cold air, are used as the u' and v' for the cold air equations. The boundary, with h = 0 for the cold air, is the front and can be followed. The J(Q) between the warm and cold air can be sensitive to the lifting of the warm air.

3. APPLICATION TO A SPECIFIC MODEL IN THE TROPICS

The wave under consideration exists on the tropical inversion and the portion of the wave to be discussed extends from about 5N to 20N with no bounding longitudes. The shape of the inversion in this region is as illustrated in Figure 1 with the lowest point of the inversion being at about 10N.

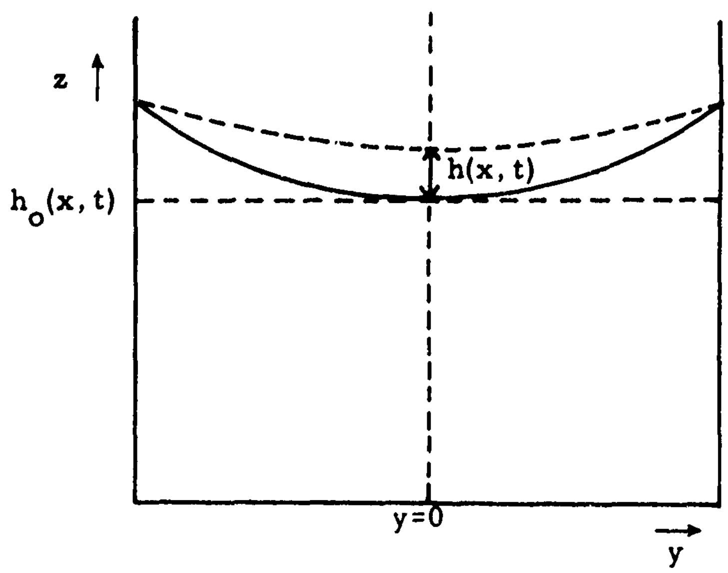


Figure 1. Simplified Shape of the Tropical Inversion

The shape of the inversion is expressed very simply as a parabolic curve

$$h = h_0(x,t)(1 + k_2 y^2) \tag{1}$$

where h is the height of the disturbed inversion surface, h_o is the height of the inversion surface at its lowest point (a function of x and t) which is taken at $y = 0$ in this analysis, k_2 is a descriptive constant (in both time and space).

The wind field is simplified by disallowing the vertical shear and north-south component of the wind. This is not an oversimplification considering the region of application of this model.

If we define the east-west components as

$$u = u' + u_L - 2y\,\gamma k_2 h_0 \tag{2}$$

we can then write the equation of motion as

$$\frac{du}{dt} = \frac{du'}{dt} - 2y\,\gamma k_2 \frac{dh_0}{dt} - \gamma \frac{\partial h}{\partial x} \tag{3}$$

where u' and u are the respective components above and below the inversion surface, γ is the buoyant gravity term defined in the list of symbols and, since the air is not particularly moist, is considered a constant in this analysis. The term $\gamma\, \partial h/\partial x$ is the driving force due to the wave and is given by

$$\frac{du_L}{dt} = -\gamma \frac{\partial h}{\partial x}$$

The horizontal speed profiles in the lower and upper fluids are considered linear and are defined respectively as

$$u_L \equiv u_0 + u_1 y,$$

and

$$u' \equiv u_0' + u_1' y.$$

Expanding Equation (3) while using the listed definitions yields

$$\frac{\partial u_0}{\partial t} + y\frac{\partial u_1}{\partial t} + \left[u_0' + u_0 + y(u_1' + u_1 - 2\gamma k_2 h_0)\right]\left[\frac{\partial u_0}{\partial x} + y\frac{\partial u_1}{\partial x}\right] = -\gamma(1 + k_2 y^2)\frac{\partial h_0}{\partial x} \tag{4}$$

The continuity equation for this system is

$$\frac{\partial h}{\partial t} = -\frac{\partial}{\partial x} u h,$$

or expanding we have

$$(1+k_2)y^2\frac{\partial h_0}{\partial t} = -\frac{\partial}{\partial x}\left[u_0' + u_0 + y(u_1' + u_1 - 2\gamma k_2 h_0)\right] h_0 (1 + k_2 y^2) \tag{5}$$

Collecting the various coefficients of the increasing powers of y we obtain

$$\frac{\partial h_0}{\partial t} = -\frac{\partial}{\partial x}(u_0' + u_0)\, h_0 \tag{6a}$$

$$\frac{\partial h_0}{\partial x}\left(u_1' + u_1 - 2\gamma k_2 h_0\right) = 0 \qquad \text{(6b)}$$

If we consider the equation resulting from the odd powers of y in (6) we note that

$$h_0\left(u_1' + u_1 - 2\gamma k_2 h_0\right) = A, \text{ a constant} \qquad (7)$$

satisfies the continuity requirement and defines the horizontal shear in the upper fluid.

If we now consider the coefficients of increasing powers of y in (4) we have the following equations.

$$\frac{\partial u_0}{\partial t} + (u_0' + u_0)\frac{\partial u_0}{\partial x} + \gamma\frac{\partial h_0}{\partial x} = 0 \qquad \text{(8a)}$$

$$\frac{\partial u_1}{\partial t} + (u_1' + u_1 - 2\gamma k_2 h_0)\frac{\partial u_0}{\partial x} + (u_0' + u_0)\frac{\partial u_1}{\partial x} = 0 \qquad \text{(8b)}$$

$$(u_1' + u_1 - 2\gamma k_2 h_0)\frac{\partial u_1}{\partial x} + \gamma k_2 \frac{\partial h_0}{\partial x} = 0 \qquad \text{(8c)}$$

Combining (8b) with (7) yields

$$\frac{\partial u_1}{\partial t} + (u_0' + u_0)\frac{\partial u_1}{\partial x} + \frac{A}{h_0}\frac{\partial u_0}{\partial x} = 0 \qquad (9)$$

If we consider the terms in (8c) individually, we see that we are comparing significances between y^2 terms in (8c) with y^0 terms in (8a). Considering the last term in the equations, we are comparing $\gamma k_2 y^2 \frac{\partial h_0}{\partial x}$ with $\gamma \frac{\partial h_0}{\partial x}$. By assuming $k_2 y^2 \ll 1$, we restrict our interest to the region where horizontal shear is less than 30 percent of the wind. The total effect of this restriction is to set both terms in (8c) equal to zero in comparison with $\gamma \frac{\partial h_0}{\partial x}$ and $(u_0 + u_0')\frac{\partial u_0}{\partial x}$.

We are then left with Equations (8a), (9), (6a) and (6b) as the set from which to compute h_0, u_0, u_1, and u'_1.*

Equations (8a and b) and (6a and b) can be stated in the form

$$\frac{d}{dt}\left(u_0 \pm 2\sqrt{\gamma h_0}\right) = -h_0\frac{\partial u_0'}{\partial x} \qquad (10)$$

as one moves with speed

$$\frac{dx}{dt} = u_0' + u_0 \pm \sqrt{\gamma h_0} \qquad (11)$$

We know from experience that this allows waves to move along the inversion and to interact and steepen. A relatively simple graphical method of computation can be used to study simple problems.

The horizontal wind shear can be computed on demand from Equations (7) and (9).

Vertical shear in the upper layer can be taken into account by proper choice of u_0'.

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*Notice that u_1 and u_1' are not needed for computing u_0 and h_0, but that they are determined by u_0 and h_0.

The first application of the equations to the atmosphere consists of a simple wave with the condition that the upper layer wind is constant downstream. This condition allows (10) to be writter

$$\frac{d}{dt}\left(u_0 \pm 2\sqrt{\gamma h_0}\right) = 0 \qquad (12)$$

with the restrictions of (11).
Our constants for this case are $u_0' = -9 \text{m sec}^{-1}$

$\gamma = 0.2 \text{m sec}^{-2}$

(negative speed is for westward movement).
The initial conditions are chosen to be

$u_0 = -2 \text{ m sec}^{-1}$
$h_0 = 1.25 \text{ km}$

If we choose the negative characteristic, we then can compute the simple wave in x, t space. The compatibility and characteristic equations become

$$u_0 + 2\sqrt{\gamma h_0} = u_{01} + 2\sqrt{\gamma h_{01}} \qquad (13)$$

and

$$\frac{dx}{dt} = u_0' + u_0 - \sqrt{\gamma h_0} \qquad (14)$$

where u_0' and h_0' are the initial conditions chosen above.

To compute the simple wave in the x, t diagram, h_0 was evaluated from radiosonde measurements at a single station in the tropics. u_0 was then computed using (13).

In Figure 2 are the results of this computation. The ordinate is time in units of hours and the abscissa is distance increasing negatively to the left in units of degrees of longitude near the equator. Each distance unit equals 111 km. As can be seen by the change in spacing of the characteristics at distances from the origin (e.g., at 10 degrees of longitude), the wave form steepens downwind with time.

If we choose the positive characteristics with the same initial conditions, we compute a slowly retrogressing wave. For the present, we shall forego discussing the retrogressing wave since we have not concentrated our search for such a wave in the data. Let it suffice to say that the simple retrogressing wave of the type described from the initial conditions shown previously is very slow moving (speed of about 10 kt eastward), and can be forecasted using the same method as for the progressing wave.

The application of this technique to a forecast of wave development can be seen in the x, t and z, t diagrams drawn from Figure 2. The upper diagram of Figure 3 is a plot of the h_0 and u_0 values resulting from the initial characteristics (t = 0) and the location of the characteristics at t = 15 hours. The lower diagram shows the time change in height of the inversion surface with the lower layer winds at a point nine degrees of longitude downwind from the initiation point. The upper and lower wave profiles result from the values of u_0 and h_0 taken along lines t = 15 hours and x = 9 in Figure 2.

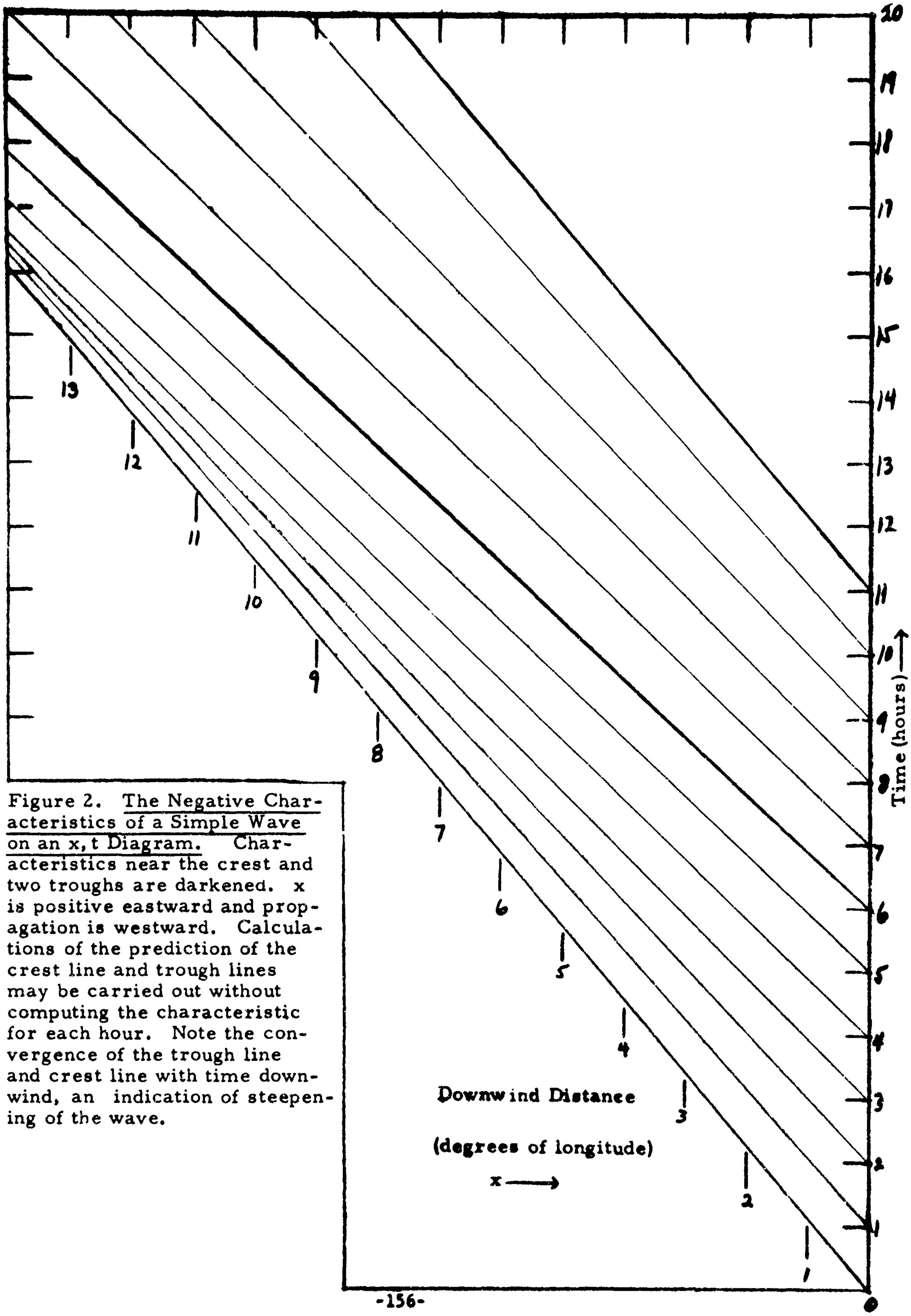

Figure 2. The Negative Characteristics of a Simple Wave on an x, t Diagram. Characteristics near the crest and two troughs are darkened. x is positive eastward and propagation is westward. Calculations of the prediction of the crest line and trough lines may be carried out without computing the characteristic for each hour. Note the convergence of the trough line and crest line with time downwind, an indication of steepening of the wave.

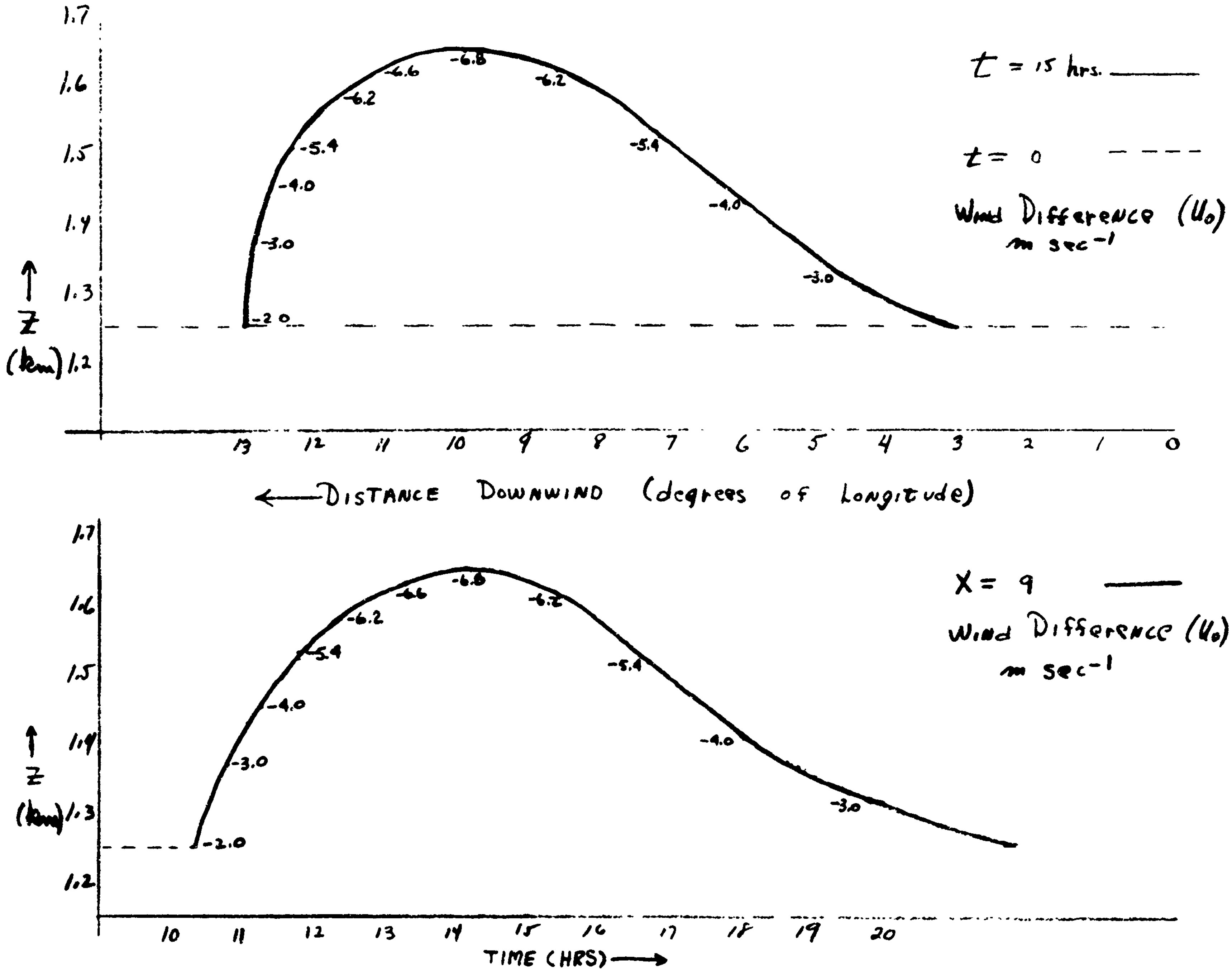

Figure 3. Cross-section from Fig. 2. Upper - x, z cross-section of computed inversion height and lower layer wind after 15 hours. Lower - t, z cross-section of computed inversion and lower layer wind at a point 9 degrees of longitude downwind of the initiation point zero.

If the abscissa and ordinate in Figure 2 were extended toward larger values of distance downwind and time, the wave would steepen and a jump would form at about 18 degrees of longitude and at about 21 hours after initiation. The data for this case indicates a jump existing at a distance of 13 degrees of longitude and 22 hours from the point of initiation. If this jump is related to our wave, then our speed is too high. This is a good indication since γ was chosen arbitrarily and the data indicates that a smaller value would be more representative.

The assumption of a constant upper layer velocity downstream resulting in the simple case shown here may also add to the difference in observed and computed wave development. Although a more sophisticated treatment of the development of waves on the inversion will be made using measured values of upper-layer shear downwind, the results of this simplified case indicates that the closely related changes in inversion height and wind are a useful forecast tool.

4. CONSIDERATION OF UNBALANCED FLOW

We now progress to the consideration of the problem of flows more useful in the study of tropical and mesoscale meteorology, and shall use Equations (1), (2), and (3)*. We still have the problem of finding h because the usual scale of measurement of upper air data is much too gross to obtain sufficient data. Of course, all measurements that are available should be used. Probably, we can find a fairly good value of the average of h over a 200-300 mile circle. And, we can use measured values of du/dt and dv/dt to find $\gamma\, \partial h/\partial x$ and $\gamma\, \partial h/\partial y$. When a good distribution of h is obtained initially we are in a position to compute later values. We also have to arrive at values for u and v. Once we are fairly sure of the distribution of h, we can continue to analyze for u and v.

In order to find u, v, and h, we assume

$$\left.\frac{du}{dt}\right|_g = \frac{d_1}{dt}\left(\frac{-g}{f}\frac{\partial H}{\partial y} - \frac{\gamma}{f}\frac{\partial h}{\partial y}\right)$$

$$\left.\frac{dv}{dt}\right|_g = \frac{d_1}{dt}\left(\frac{+g}{f}\frac{\partial H}{\partial x} + \frac{\gamma}{f}\frac{\partial h}{\partial x}\right)$$

where

$$\frac{d_1}{dt} = \frac{\partial}{\partial t} + u_g\frac{\partial}{\partial x} + v_g\frac{\partial}{\partial y}$$

where the subscript g denotes geostrophic.

We now set

$$f(v-v') = \left.\frac{du}{dt}\right|_g + \gamma\frac{\partial h}{\partial x} + F u_g\sqrt{u_g^2 + v_g^2}$$

$$f(u-u') = -\left.\frac{dv}{dt}\right|_g - \gamma\frac{\partial h}{\partial y} - F v_g\sqrt{u_g^2 + v_g^2}$$

We find u, and v at two times by this method and we call them $u_1(t)$, $v_1(t)$. We then set

$$f(v-v') = \frac{du_1}{dt} + \gamma\frac{\partial h}{\partial x} + F u_1\sqrt{u_1^2 + v_1^2}$$

$$f(u-u') = -\frac{dv_1}{dt} - \gamma\frac{\partial h}{\partial y} - F v_1\sqrt{u_1^2 + v_1^2}$$

*These equations are found in Section 1.

When this has been accomplished, we should have good values of u and v and we are in a position to recompute du/dt and dv/dt and continue the computation.

These diagnostic and analysis tools are just as important and probably more difficult to compute than the prediction equations.

The method of computation is important in consideration of computational stability. The obvious physical stability criterion for these equations is given by

$$\frac{\Delta x}{\Delta t}, \frac{\Delta y}{\Delta t} > \sqrt{\gamma h}$$

We are reasonably sure that computations governed by this criterion will be stable. Thus, if $\sqrt{\gamma h} \geq 60$ m.p.h. (a new value), then we can work over a grid of 40 mile squares with 30 minute steps. If we wanted to work on 10 mile squares, we would need 7-1/2 minute time steps.

This is a complete and comprehensive set of equations for two-dimensional time dependent flow under an inversion. They contain the capability of portraying all sorts of fronts, gravity waves, wave cyclones, hydraulic and pressure jumps, squall lines, vortex sheets, and hurricanes. The boundaries pose special problems especially if they are lines at which the inversion height is zero. Many special cases in both the steady and unsteady state have been studied in the past.

DISCUSSION ON FREEMAN AND GOLDMAN'S PAPER

HENRY: I have just one quick question here. The bubble breaks off. How far would you be willing to let that move on further south? As far as the equator?

FREEMAN: Well, from experience, yes. From theory, I don't know. Over by Australia I'm fairly sure those come on down to the equator. Well, to 5 degrees, and I'm fairly sure, to the equator. I am a little bit doubtful - I went from 5 degrees to the equator, and I was a little bit doubtful sometimes whether they really reached the equator, or not.

HENRY: I would like to see them hit the South American coast.

FREEMAN: Well, I'm with you - I think they do.

ESTOQUE: Do you expect any weather to be associated with this kind of disturbance?

FREEMAN: Which one?

ESTOQUE: The inversion wave?

FREEMAN: The wave on the inversion - the last one under discussion? Increased convective activity.

ESTOQUE: Have you found any evidence?

FREEMAN: Mr. Goldman, is there weather with any of the 3 or 4 we've studied?

GOLDMAN: The wave moves faster than the weather. In other words, the wave is moving at a speed much greater than the showers themselves. It seems to go through and then the showers seem to follow behind, but the wave is moving much faster than the weather. Those are the three cases we have looked at so far. Now there may be other cases where the speed of the wave and maybe the weather is---.

FREEMAN: The little analysis that we did here gives you speeds of 40 and 50 knots for the waves. These are what Dr. Portig calls the fast moving ones.

ESTOQUE: How fast do the waves travel? This is a gravity wave, isn't that right?

FREEMAN: Yes, anywhere between 40 and 60 knots.

ESTOQUE: How about the weather?

FREEMAN: Wind speed?

ESTOQUE: Wind speed.

GENTRY: I would like to ask one question. I think I missed part of your explanation there at the board. I understood you to say that when you got into the area where the west winds got weaker, the front, or before the bubble broke off, started moving faster. Now, I think I misunderstood you.

FREEMAN: Oh, no. I said that. That is the result of Margules formula. When the west wind is weaker and you were balanced for a certain west wind, if you keep that same slope of the inversion and move into a place where the west wind is weaker, your flow will be unbalanced and will shoot out. Now, also the faster the east wind, the more the frictional effect drives the air, you see. If your east wind becomes stronger, your transport becomes greater, so there is another factor to move it faster. It's a highly non'linear action and I think that's one reason we always miss it. The cold air shoots to the south very fast because two things are working to make it go faster.

GENTRY: You are talking strictly about the movement of the wave then, aren't you, because the air is moving against the wind under the conditions as I understood you to describe them.

FREEMAN: The boundary that I have drawn there is the top of the cold air, and where I have the horizontal lines, they represent the ground, so that the wind here is strictly east or west, nothing else. Then there is a surface wind that's a little bit different that is brought about by friction, but what I was calling the wind was basically the gradient wind. It's either east or west, and there are two things that move that surface. Really in the theory we are only taking good account of one of them, and that is the frictional transport of the air across the height contours or the isobars that is brought about by friction. In practice there is also the lack of balance due to Margules formula that occurs at the same time.

KRAUS: Well, actually I have two questions. Is there any observable surface pressure disturbance associated with your disturbance there?

FREEMAN: Yes, this one we discussed last.

KRAUS: No, I mean the separated role.

FREEMAN: I would say yes. Our synoptic investigation of this problem is scant. It is not what we were put to work on. It's what if you live near Louisiana, you would call a laneyap. It's something we got free because we were working on this problem, but we do not feel justified in a synoptic investigation of it on our present contract.

KRAUS: I have not been able to understand quite what is the physical cause which moves this thing along.

FREEMAN: The physical force is the transport of air toward low pressure when you have frictional interaction with the ground. That moves the air and when the air moves, that gives you convergence and divergence.

Structure of rain in Entebbe, Central Africa

by

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1. Introduction

The measurements on the structure of precipitation we made in Axel Heiberg Land 80^0 north latitude, at Shannon/Ireland $52^041'$ north latitude, at Wyck on Föhn $54^040'$ north latitude, at Karlsruhe $49^000'$ north latitude, at Ispra/Italy $45^040'$ north latitude, and at Palma/Mallorca $39^035'$ north latitude have been completed by measurements in Central Africa at Lwiro/Lake Kivu $02^015'$ south latitude and Entebbe/Lake Victoria $00^003'$ north latitude. This report will give the results of Entebbe.

2. Time and Location

The measurements at Entebbe took place from March 19 to April 12, 1964 and thus lie at the beginning of the rainy season which reaches its maximum in April. Simultaneously with the measurements at the ground we were able to make also measurements from the airplane and thus to lay hold of the cloud structure which, however, will not be dealt with in this report.

3. Material

44 rains have been covered by 2004 measurements with 207854 single drops being measured. In doing this, we fell back

upon Wiesner's method which uses a filter dusted with eosine and talcum from the underside and exposes it to the rain. The spots created thereon are a measure for the drop diameter. Measurement of the spots is effected with the aid of the particle size analyzer TGZ 3 of C. Zeiss, Oberkochen, further evaluation by the electronic computer IBM 1620. As a result the following values are obtained: N, $\bar{d}$, d_x, W, I, R (Table 1).

4. Results

4.1 Climatic survey

The 44 rains within 25 days preferably fell between midnight and noon with maxima between 5 to 6 and 8 to 9 hours (Table 2). Their quantity amounted to a maximum of 56,2 mm, their duration to a maximum of 318 minutes (Table 3). Rains of lower intensity lasted up to 150 minutes, rains of highest intensities from 60 to 330 minutes. In general, rains up to 30 mm/h and a 90-minute duration prevailed.

4.2 A survey of the single measurements

The 2004 single measurements which permitted to recognize the structure of the rains have been made during the 44 rains as mentioned above at intervals of 1 - 2 minutes (Table 4). The intensity of these single measurements has two maxima at 0,0 to 0,6 and at 1,21 to 3,0 mm/h and very often precipitations of more than 12 mm/h. The allotted drop numbers N vary between a few and some 10.000 drops with two pronounced maxima at 100 to 200 and 500 to 700 drops. It is obvious that light rains are coupled with low drop numbers, heavy rains with high ones and vice versa.

The classification according to the spectrum width (d_x) (Table 5) reveals a well-marked concentration point of the spectrum width for each rain intensity, at the same time, however, it shows how wide the limits are within which the spectrum widths may scatter. As clear results must be noted: small spectra are allotted to rains of low intensity, wide spectra to those of high intensity; drops of diameters exceeding 4,5 mm are very seldom (0,5 %), as upper limit a diameter of 4,0 mm can be expected. For further treatment we combine the spectrum widths into five groups (Table 6), the mean groups appearing with rather the same frequency, the smallest and the largest groups with about half the frequency. Spectrum width and rain intensity are more strongly associated, the scattering of the values is clearly maintained, it being typical, for instance, that with spectra of 2,26 to 3,00 mm diameters all rain intensities have been measured.

The correlation between mean drop diameter $\bar{d}$, spectrum width d_x and precipitation intensity I, studied many times already, can very beautifully be demonstrated by the present material (Fig. 1). For each intensity point clouds are obtained which scatter within a wide range, i.e. there is no distinct and close correlation between the mean drop diameter and the spectrum width. When lines of equal frequencies from 2,5 to 2,5 % are plotted (the outmost line always amounts to 2,5 %) concentration points of high frequencies around 7,5 or 10 % of all observations are obtained. From the ligth (0,0 - 0,6) to the mean (3,01 to 6,0) rain intensities these points of concentration shift rather uniformly from small $\bar{d}$ and d_x values to large ones, approximately from $\bar{d}$ = 0,8 to $\bar{d}$ = 1.3 and accordingly from d_x = 1,5 to d_x = 3,0. However, express mention must be made that the share of these concentration points in all measurements of the associated rain intensities comes up to 10 % at a maximum. For the rain intensities exceeding 6,00 mm/h $\bar{d}$ and d_x do no longer shift which can only be explained by the assumption that the drop number at virtually unchanged $\bar{d}$ and . becomes the determining value for these intensities.

4.3 Special investigations

For an ideal rain with a single drop size we may plot a diagram (Fig. 2) which contains all important values $\bar{d} = d_x$, N, W and I. Unfortunately this ideal rain will never occur , so that we must take instead rains with wide spectra where $\bar{d}$ is unequal d_x. In order to show the consequences which result from rains composed of drops of different sizes we plotted the diagrams of the actual rains at Entebbe and, in part, compared them with the results of other measuring points.

In the W-N diagram (Fig. 3) the scattering of the values for the mean drop diameter $\bar{d}$ is relatively low and the lines of equal $\bar{d}$ can easily be depicted. The number of values which do not lie in the allotted range is low, the curves coincide surprisingly well with those of the ideal rain except for the range of low water contents and/or small drop numbers and small drop diameters. Similar measurements on Mallorca (Fig. 4) showed less deviations from the ideal rain (red curves) than at Entebbe and in the opposite direction.

Analogously to Figure 3 a diagram may be plotted for the intensity (Fig. 5). Here, too, the number of the values lying outside the lines is very small. In the intensity range around 2,5 to 5,0 mm/h the deviation from the ideal rain is very low, in the ranges of higher and lower intensities, however, it is relatively large. Still more pronouncedly, the same phenomenon has been observed during the measurements at Karlsruhe. Both figures show that with the intensity of precipitation remaining constant the number of drops varies by the factor 17 and the water content accordingly by the factor 2,1. These factors vary from measuring point to measuring point and seem to be values subject to meteorological and orographical influences. This, however, means that the size of the rain drops and the spectrum of the rains differ over long ranges.

The consequences, these results lead to, may be seen from the two last figures which represent the radar reflectibility in correlation with the spectrum width d_x and the water content W (Fig. 7) and /or the rain intensity I (Fig. 8). The solid lines have been plotted as lines of the concentration points for a certain spectrum width and thus simultaneously give the range of the most frequent occurrence. Thus the spectrum widths exceeding 3,0 mm cover water contents from 0,04 to 6 g/m^3 and at the same time radar reflectibilities from 700 to 200.000 and the spectrum widths lower than 1,25 water contents from about 0,001 to 0,15 g/m^3 and radar reflectibilities from about 1 to 200. One phenomen, however, is revealed very distinctly: depending in the spectrum width, the same water content can cause a radar reflectibility which fluctuates by the factor 50 and, vice versa, the same radar reflectibility can be caused by water contents which differ by the factor 10.

Since, normally, we only measure the rain intensity, the fig. 8 makes again connections with the meteorological values generally known. Rain intensities of 1 mm/h may occur with all spectrum widths from less than 1,25 to morethan 3,00 mm. Intensities of 10 mm/h are restricted to spectra exceeding 1,75 mm, those of 100 mm/h to spectra greater than 3,00 mm. In the range of the most frequent rain intensities from 0,61 to 3,0 mm/h (also see Table 4) all spectrum widths are contained in the precipitation and the allotted radar reflectibilities fluctuate for the same intensity by the factor 10 and for the intensity range by the factor 65. Only with very high rain intensities exceeding about 30 mm/h the scattering of the radar reflectibility is reduced to the factor 2,5 and associated to the widest spectra.

5. Summary

2.004 single measurements of Central-African precipitations give an image of their structure and of the correlation of the individual values. The main deviation from the ideal rain which has one drop size only is relatively low, the scattering of the single values,

however, high. The main drop diameter and the spectrum width show a well defined association with the water content and the intensity of the rain. The calculation of the radar reflectibility reveals that it may vary by the factor 10 with constant rain intensity as a function of the spectrum width and, vice versa, that the same radar reflectibility may be subject to rain intensities which vary by the factor 6.

Table 1 Symbols

Number of drops	N	$[N/dm^2 \cdot min]$
Mean drop diameter	$\bar{d}$	$[mm]$ $\quad \bar{d} = \sqrt[3]{\frac{\sum n_i d_i^3}{\sum n_i}}$
Width of the spectrum	d_x	$[mm]$
Water content	W	$[g/m^3]$
Intensity of rain	I	$[mm/h]$
Radarreflectibility	R	$[mm^6/m^3]$

Table 2 Probability of rain in % for every hour of day

daytime	-1	-2	-3	-4	-5	-6	-7	-8	-9	-10	-11	-12
probability	8	20	20	32	36	48	40	24	36	16	12	8
daytime	-13	-14	-15	-16	-17	-18	-19	-20	-21	-22	-23	-24
probability	4	-	-	-	4	-	-	-	-	-	-	-

Table 3 Amount of rain R in [mm] and duration D [min] of the 44 rains according to the raingauge. Number of cases.

D \ R	-0,6	-1,2	-3,0	-6,0	-12,0	> 12,0	Σ
0 - 30	12	2	2	-	-	-	16
- 60	5	1	1	1	1	-	9
- 90	2	-	3	1	-	1	7
-120	-	-	2	1	-	3	6
-150	-	1	-	-	-	-	1
-180	-	-	—	-	-	-	-
-210	-	-	-	-	1	1	2
-240	-	-	-	-	-	-	-
-270	-	-	-	-	-	-	-
-300	-	-	-	-	1	1	2
-330	-	-	-	-	-	1	1
Σ	19	4	8	3	3	7	44

Table 4 Intensity of rain I [mm/h] and number of drops N [N/dm^2·min]. Number of measurements.

N \ I	0,0 : 0,60	0,61 : 1,20	1,21 : 3,0	3,01 : 6,0	6,01 : 12,0	> 12,0	Σ
-100	308	13	-	1	-	-	322
-200	237	72	21	-	-	-	330
-300	102	82	54	5	1	-	244
-400	31	53	70	16	2	-	172
-500	10	37	56	23	2	-	128
-700	3	39	122	78	6	1	249
-1000	-	12	80	70	12	3	177
-2000	-	-	17	77	87	25	206
-3000	-	-	-	2	14	46	62
-5000	-	-	-	-	6	50	56
-7000	-	-	-	-	-	21	21
-10000	-	-	-			17	17
>10000						20	20
Σ	691	308	420	272	130	183	2004

Table 5 Intensity of rain I [mm/h] and width of the spectra d_x [mm]. Number of measurements.

d_x \ I	0,0 : 0,6	0,61 : 1,2	1,21 : 3,0	3,01 : 6,0	6,01 : 12,0	>12,0	Σ
-0,75	4						4
-1,00	82	6					88
-1,25	122	27	8				157
-1,50	161	53	45	9			268
-1,75	133	37	65	17	2	1	255
-2,00	78	47	65	32	8	8	238
-2,25	60	46	49	30	21	7	213
-2,50	29	44	66	42	37	24	242
-3,00	18	37	92	73	38	76	334
-3,50	3	11	29	55	19	48	165
-4,00	1		1	13	4	12	31
-4,50				1	1	5	7
>4,5						2	2
Σ	691	308	420	272	130	183	2004

Table 6 Intensity of rain I [mm/h] and width of the spectra d_x [mm/h] in groups. Number of measurements.

d_x \ I	0,0 : 0,6	0,61 : 1,2	1,21 : 3,0	3,01 : 6,0	6,01 : 12,0	> 12,0	Σ
0-1,25	208	33	8				249
1,26-1,75	294	90	110	26	2	2	523
1,76-2,25	138	93	114	52	29	15	451
2,26-3,0	47	81	158	115	75	100	576
>3,0	4	11	30	69	24	67	205
Σ	691	308	420	272	130	183	2004

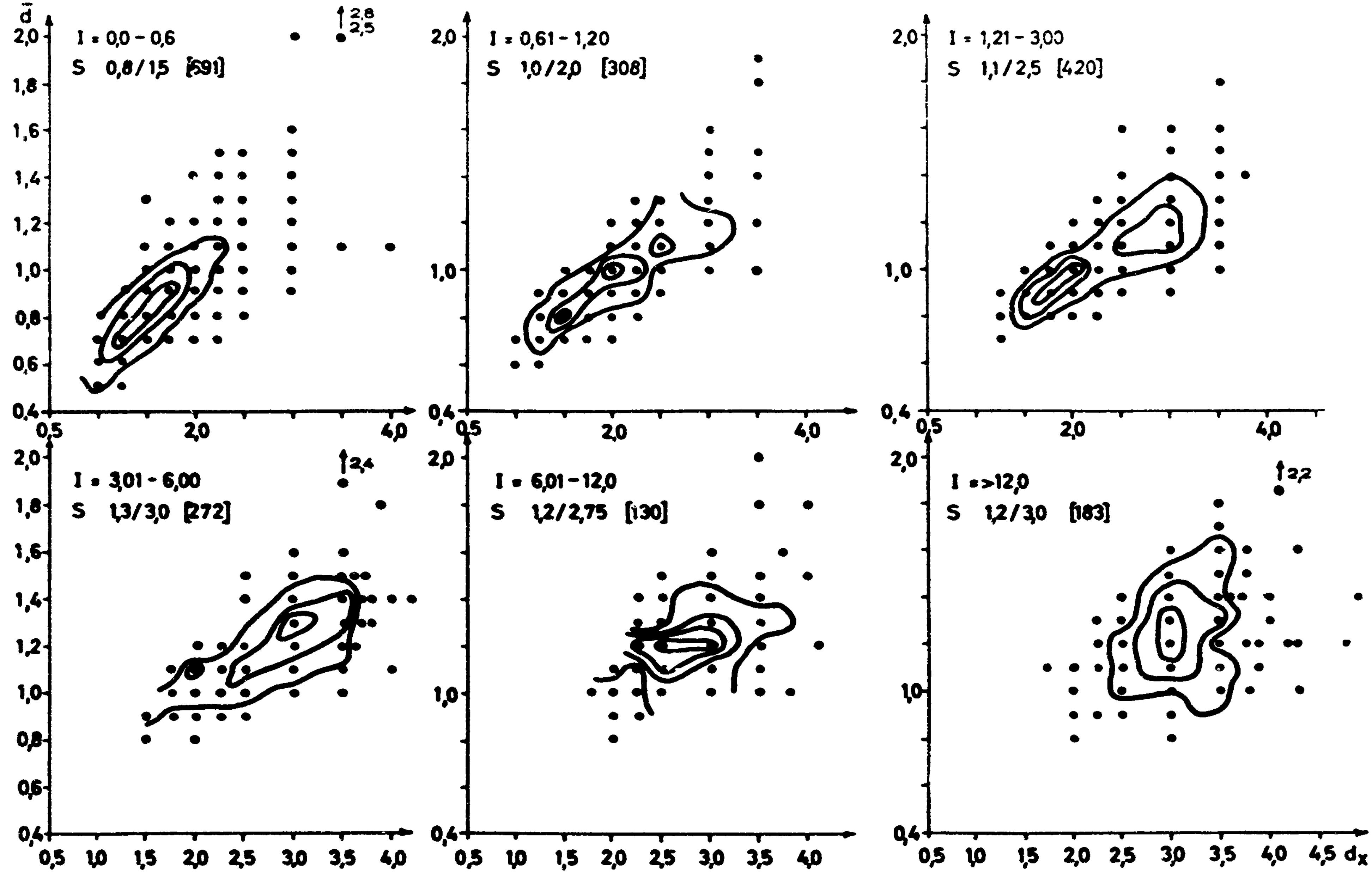

Fig.1 Mean drop diameter $\bar{d}$, width of the spectrum d_x and intensity of rain I.

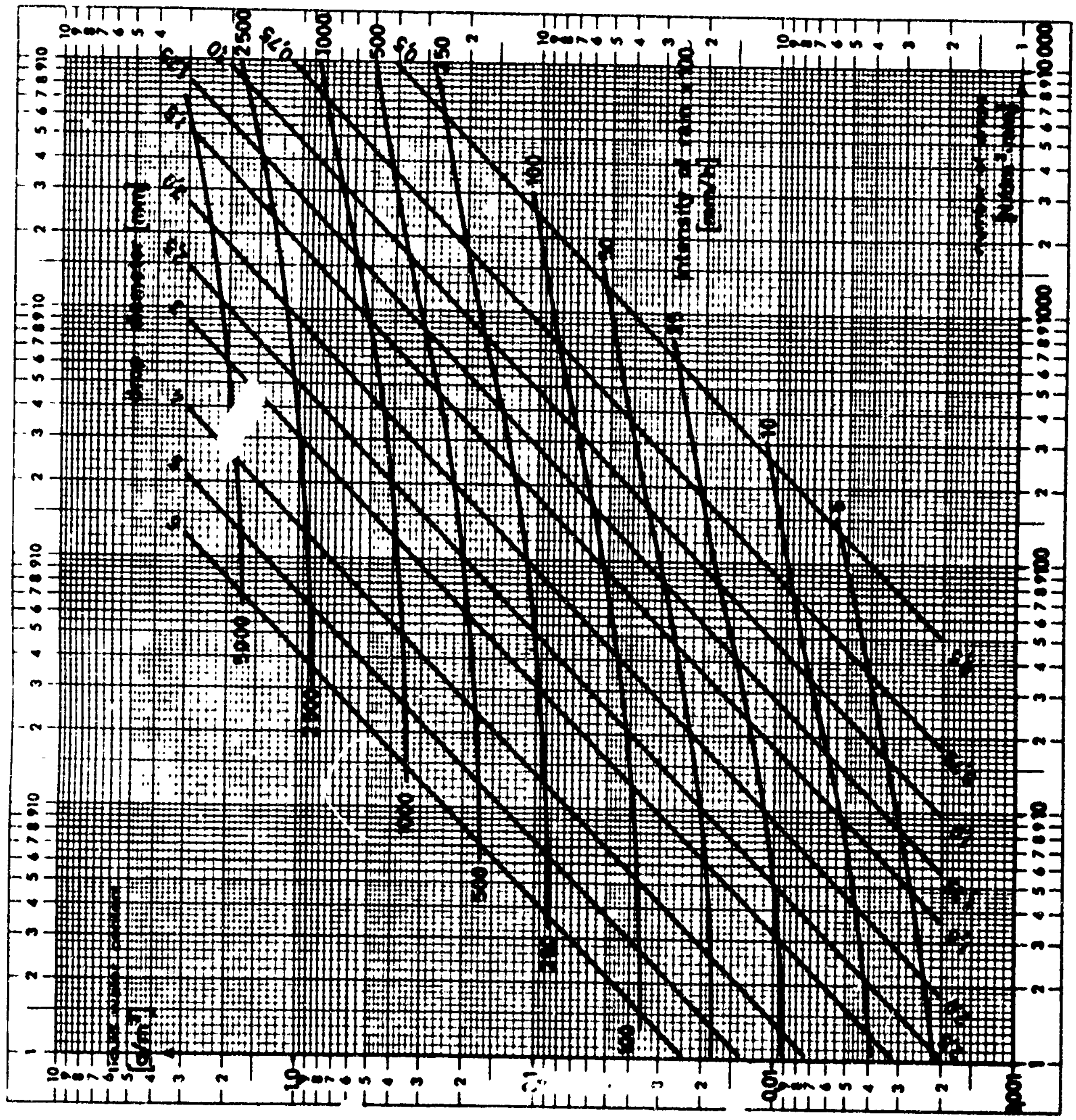

Fig. 2 Diagram of "ideal" rain.

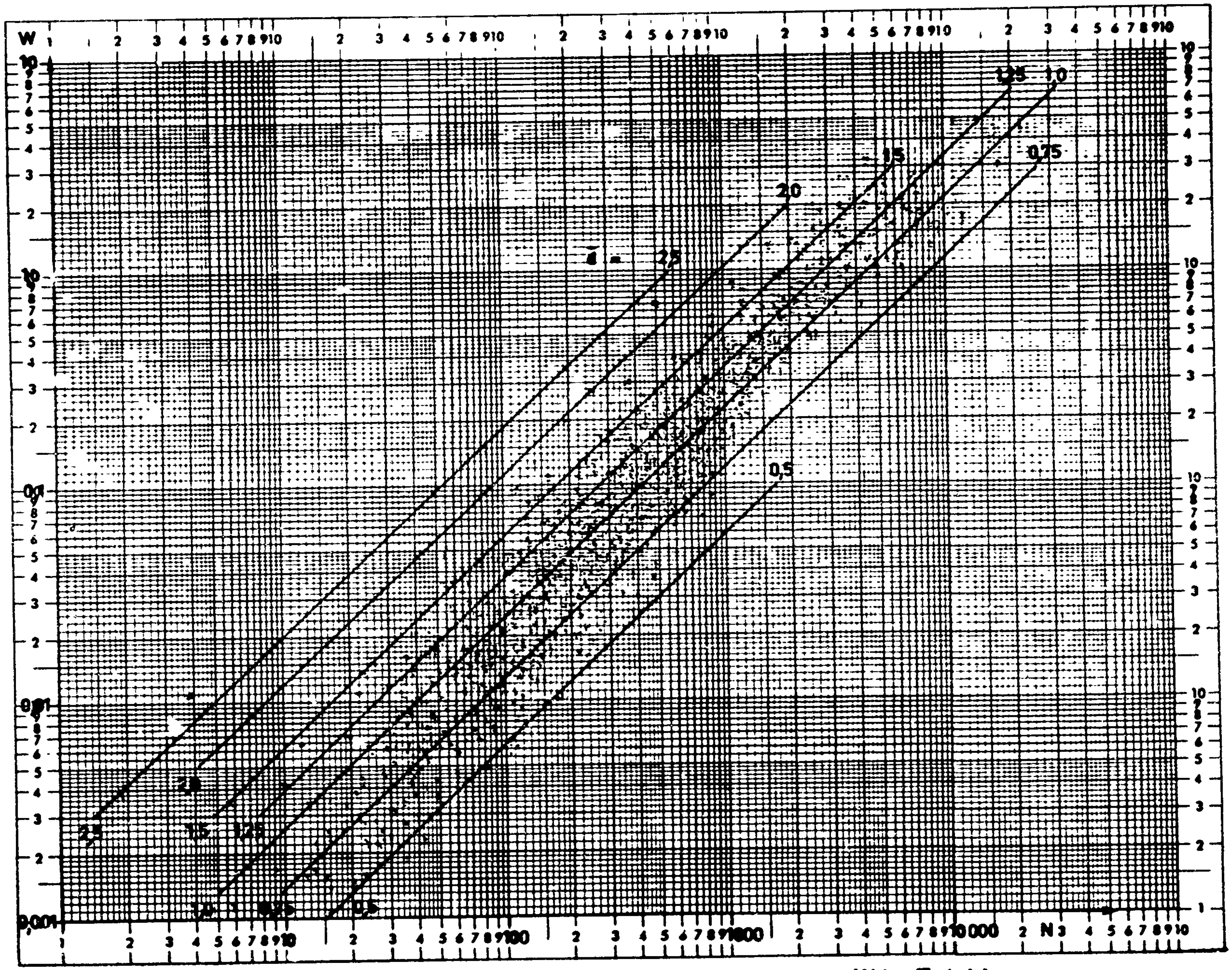

Fig. 3 Drop size $\bar{d}$, number of drops N and water content W in Entebbe.

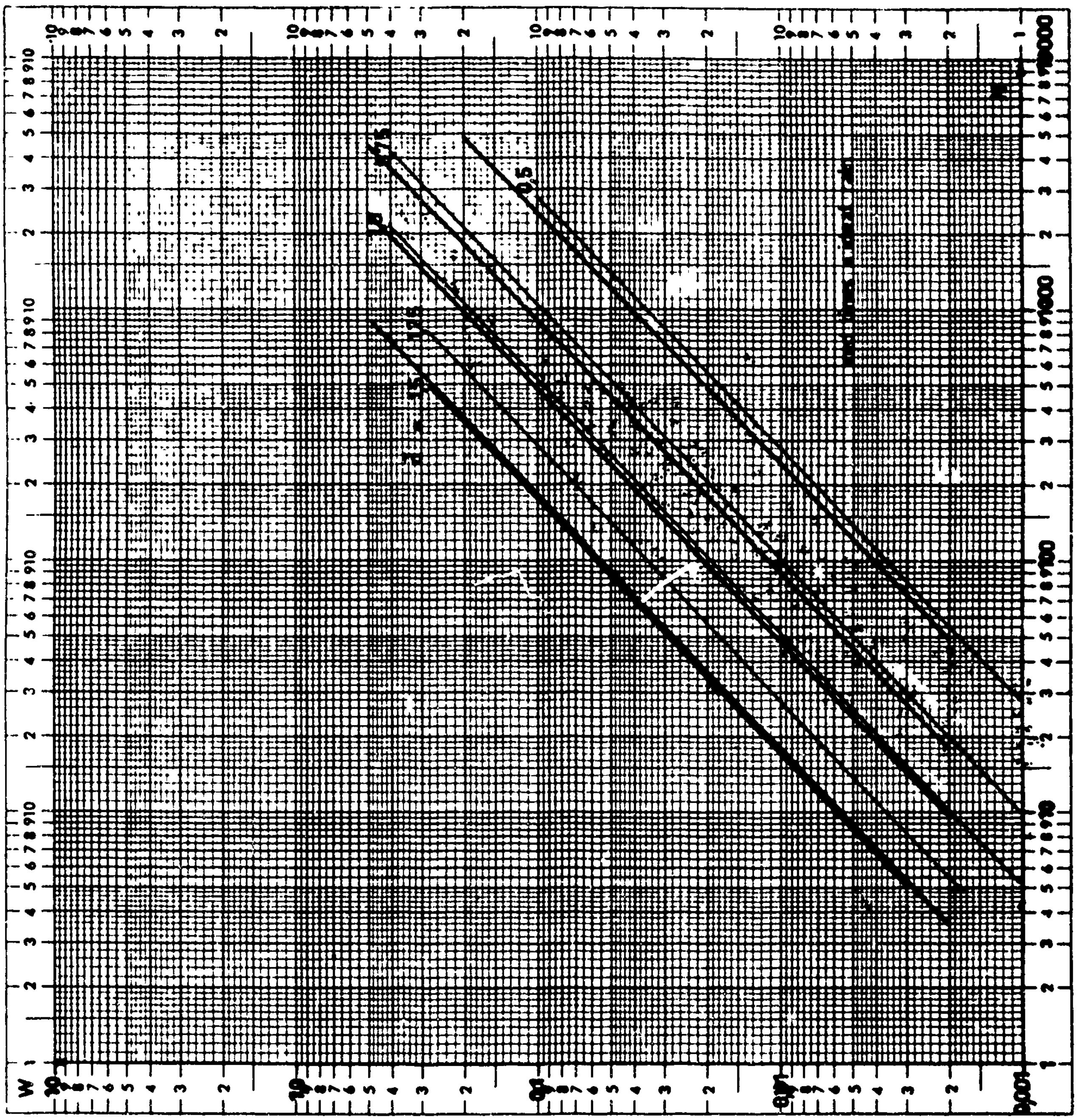

Fig.4 Drop size $\bar{d}$, number of drops N and water content W in Mallorca.

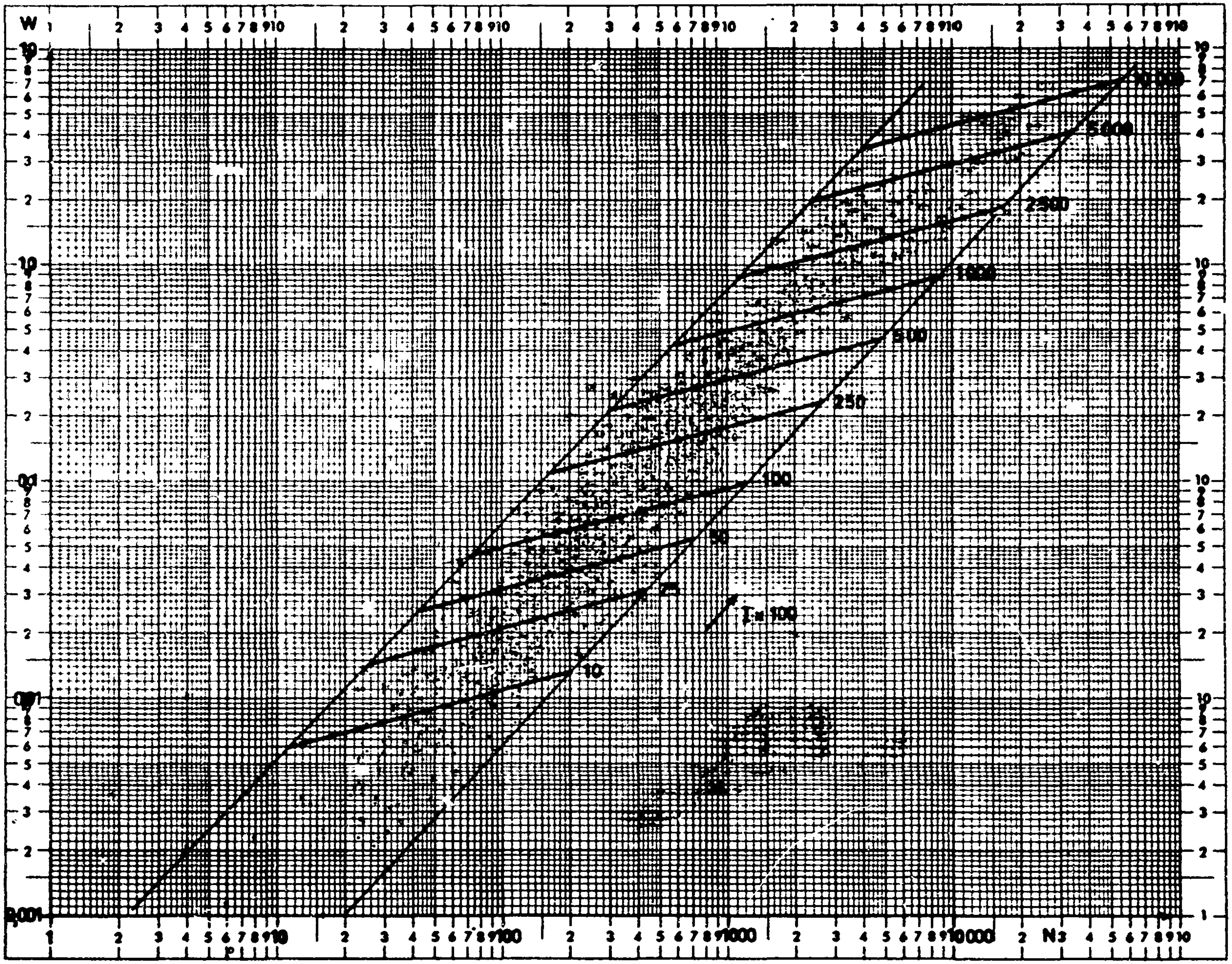

Fig.5 Intensity I, number of drops N and water content W in Entebbe.

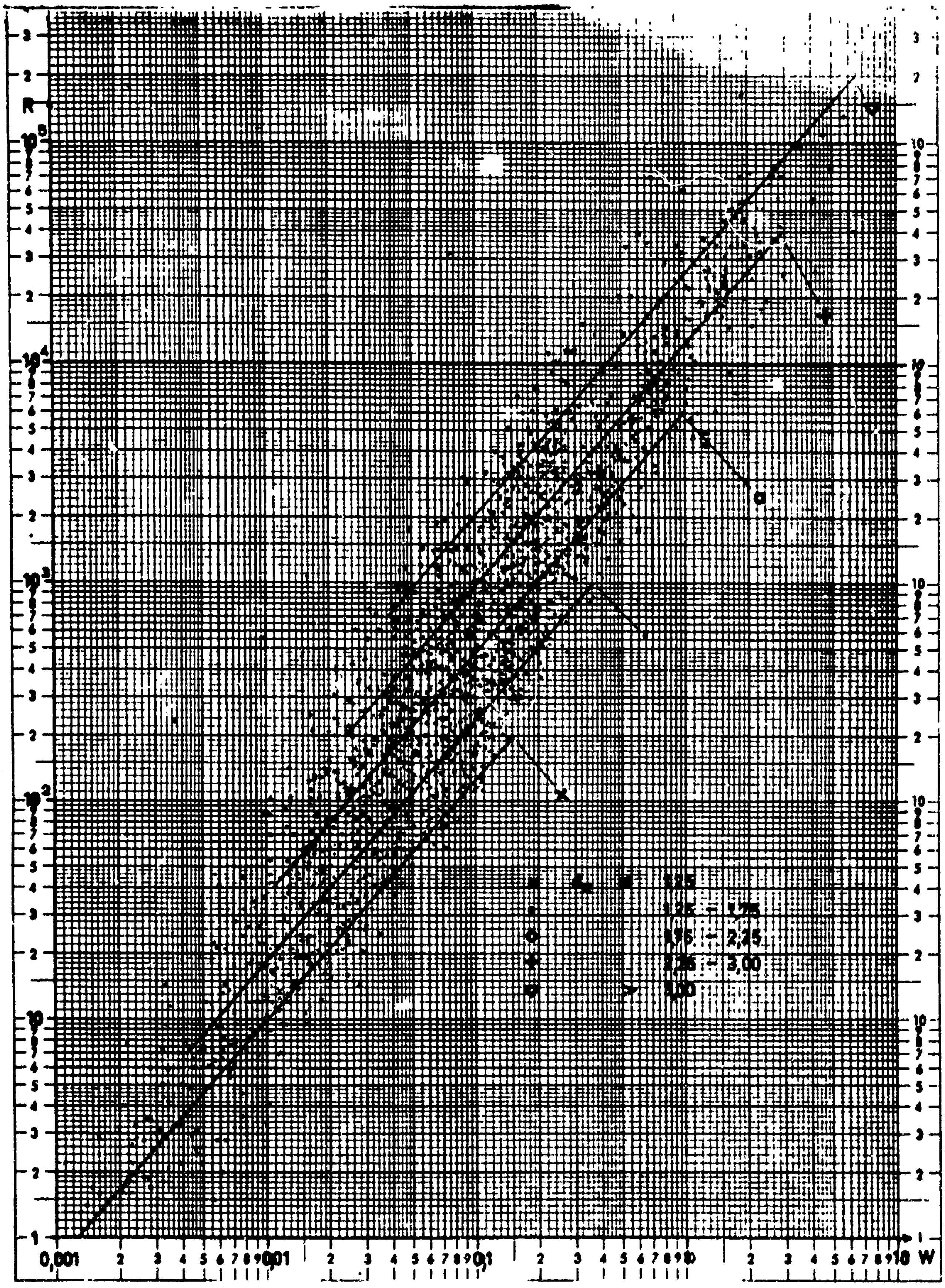

Fig.7 Radarreflectibility R, water content W and width of the spectra d_x.

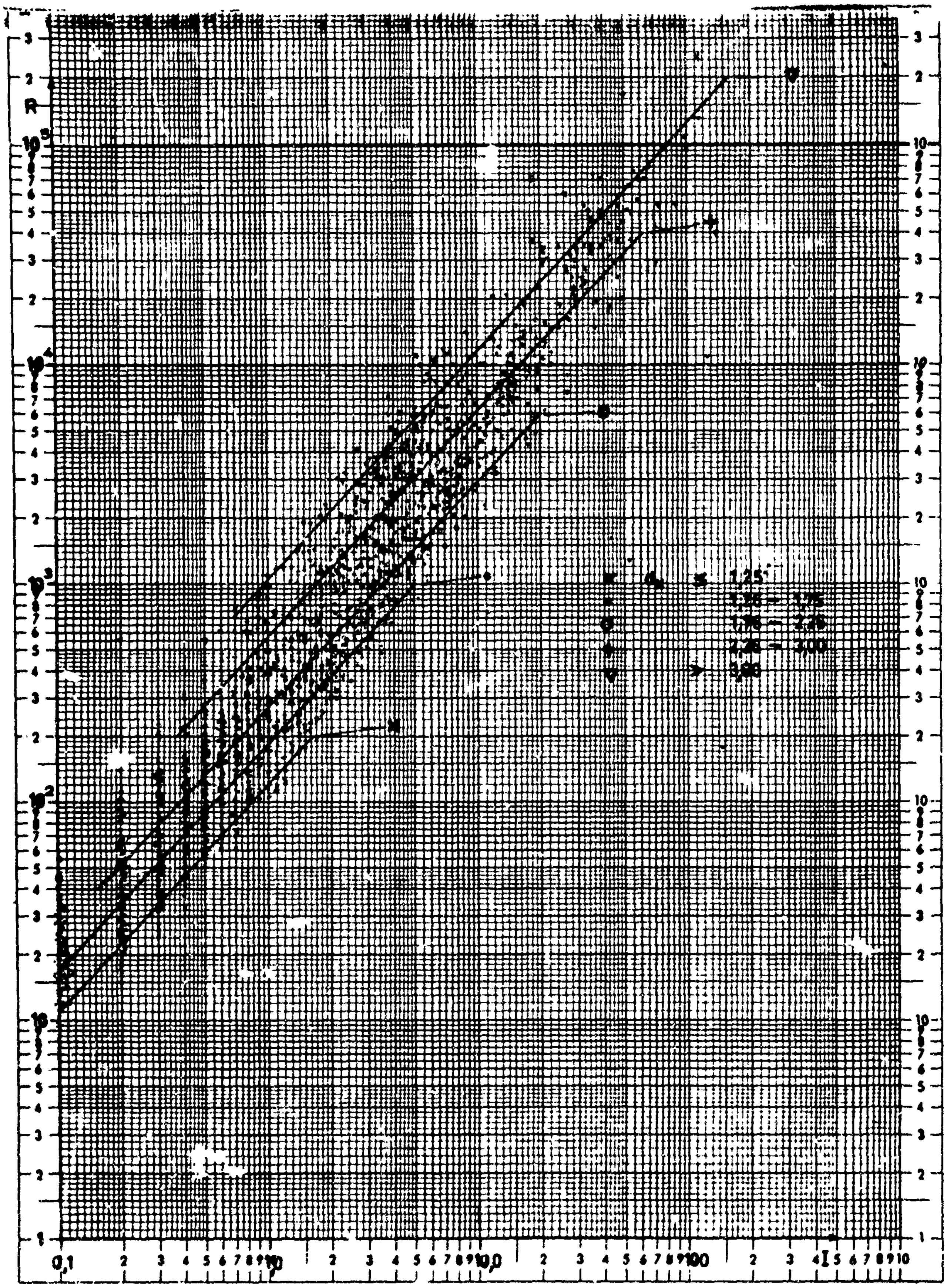

Fig.8 Radarreflectibility R, intensity of rain I and width of spectra d_x.

DISCUSSION ON DIEM'S PAPER

HISER: This is a subject that I have been concerned with for a long time. Mr. Diem, I think you have done a very fine piece of work here, but I wonder if you have some suggestion as to what to do with intermediate rainfall rates. How do you handle this problem that has been bothering radar meteorologists for years - the fact that a wide expanse of Z values represent one rainfall rate, or the converse as you want to look at it? This is the result of drop sizes, as we saw in your work, but do you have any suggestions here?

DIEM: Why this is so?

HISER: No, not why, but how we might resolve this problem in radar measurement of rainfall.

DIEM: Oh. We have made some measurements in Germany (you know perhaps Flacke-?) on radar reflectivity and rainfall, and he didn't get any close connection between radar reflectivity and rainfall. Therefore, we also made these measurements. We calculated radar reflectivity every one minute from our exact measurements using the well-known formula and said to these people, "look here, there is a change with rain spectrum".

HISER: What we need is some way of sampling raindrop sizes in each and every rainstorm. The people at the Illinois Water Survey at the University of Illinois have attacked this problem by categorizing the type of rain, whether it was thunderstorm rain, stable type, etc.

DIEM: I am sorry this is not possible. According to our measurements, we never can say from a spectrum of Z size from groups of drops that this is a shower or this is not a shower. Our most expensive materials is from Karlsruhe. We are now working on this material ahead to some 10,000 single measurements, and we have wide spectra also in showers as in normal rains. We also have a small spectrum in showers. This is a terrible thing that you also have small spectras in showers, spectras which only contain drops from 3 to 5 millimeter diameter and what was for us a very important thing. We don't find large droplets in Africa. We expected to have droplets from 8 millimeter diameter in Africa. Nothing! The highest diameter has been 5.1 millimeter. Also, in the Congo there was very, very heavy showers.

GERRISH: Just a brief comment. Recently we have used the Z-R relationship, Z = 300 R, 1.4, or I believe you used "I" for rainfall intensity. We've used this as a typical moist season equation for the Miami area developed on raindrop data from the Illinois Water Survey camera studies in the past here at Miami. Would you care to comment on this as to whether it would be suitable, or not, in Africa, and if not, what would you say would be a typical climatological Z-R relationship, or Z-I relationship which would be suitable there?

DIEM: I have not understood. Please repeat the question.

GERRISH: Well, we've used Z equals 300 R to the 1.4 power as a Z-R relationship between Z reflectivity and R rainfall rate. How does this compare with your results in Africa? Would you care to modify it to some extent? This looks like a good value here for South Florida. What would you say would be good for Africa?

DIEM: We have calculated all these equations for all our rains, but I haven't shown this here. This is nearly the same. We have a factor between 100 and 300-500, 350 and the exponent will be between 1 1 and 1.5 for all these equations, or drop points. This is nearly the same thing.

FREEMAN: Well, I would think that the first suggestion someone would make in answer to Mr. Hiser's question about what would you do to distinguish the size of the drops, would be to get a spectrum of radar frequencies and use whatever difference in response to drop size to frequency that you could get from that. Of course, this would right away triple or quadruple the cost of an experiment, but in the most significant place. That would be the first reaction, the next thing to check. If you're sure you can't do it with one radar set now, then the natural reaction is to try for five.

Kinematics and Dynamics of Tropical Precipitation

by

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ABSTRACT

The work reported deals with the first phase of a numerical experiment in moist cloud convection including the precipitation phase. It is confined to the special case of shallow dry convection of the roll-type and concerns the development in time of a buoyant bubble in neutral surroundings. The evolution of events studied span over 20 minutes beginning with a maximum temperature excess of $0.38°C$ over the neutral surroundings at 600 m above the ground and ending by the time the temperature maximum has been reduced to $0.13°C$ and is approaching the upper boundary (3000 m). At an early stage, the bubble takes the shape of a mushroom cloud with a slim stem and a rounded top which is characterized by a concentration of isotherms. The associated circulation increases first quadratically with time, but later it becomes near linear and shows deceleration at the end of the twenty minute period.

Of particular interest is the pressure field that is induced by the buoyant bubble. It creates an inflow along the lower boundary and accelerates the inflowing air upwards along the stem of the rising bubble. In the upper part of the bubble, the pressure gradient acts against the buoyance force and the effect is a form drag similar to that experienced by a body moving through a viscous fluid. The pressure is far from being hydrostatic.

1. INTRODUCTION

Numerical experiments in dry-convection have been conducted by Lilly [1] and Ogura [2], and the latter has recently extended his work to include the cloud phase [3]. The work reported here is the first phase of a more comprehensive experiment on tropical cloud convection including the precipitation phase and the bulk effects of microphysics of cloud and precipitation. These effects have been previously studied by Kessler et al. by means of a purely kinematic model under Contract DA 36-039 SC 89099 and reported elsewhere [4].

The partial results presented in Section 3 are similar to those of Lilly and Ogura but go further in that the pressure field induced by a dry thermal and the associated circulation is discussed. Continued work is aimed at a gradually increased complexity and will include both shallow and deep moist convection.

2. A NUMERICAL EXPERIMENT IN CLOUD CONVECTION

For the purpose of simulating the convective processes, we assume a model atmosphere initially at rest and bounded by two rigid horizontal planes a distance $H(= 3\text{km})$ apart of which the lower one coincides with the coordinate surface $z = 0$. Laterally, the atmosphere is bounded by two rigid vertical planes a distance $2L(= 6\text{km})$ apart in the x-direction but is of infinite extent in the y-direction (a roll cloud). The initial conditions are composed of a) a basic state, and b) superimposed perturbations. The physical variables characterizing the basic state are functions of the vertical coordinate z only, while the perturbations are functions of x , z , and t . All quantities are assumed independent of y at all times, and symmetry about the plane $x = 0$ is imposed. This permits limitation of the numerical experiment to the domain $x \geq 0$ (right-hand half of the roll cloud).

The physical basis for the numerical experiment has been described in detail in a previous report [5], so this account will be limited to the theory of long-roll dry convection in neutral surroundings for which results have just become available. As shown by Ogura and Phillips [6], we may ignore variation in density for the case of shallow convection and apply the Boussinesq approximation. With this and other simplifications, mentioned above, the equations appropriate to the problem at hand are

$$\frac{\partial u}{\partial x} + \frac{\partial w}{\partial z} = 0 \tag{1}$$

$$\frac{\partial \theta}{\partial t} + \mathbf{v} \cdot \nabla \theta = 0 \tag{2}$$

$$\frac{\partial \zeta}{\partial t} + \mathbf{v} \cdot \nabla \zeta - \frac{g}{\theta_0} \frac{\partial \theta}{\partial x} = 0 \tag{3}$$

$$\frac{\partial^2 p}{\partial x^2} + \frac{\partial^2 p}{\partial z^2} - \frac{g\rho_0}{\theta_0} \frac{\partial \theta}{\partial z} + \rho_0 \operatorname{div}(\mathbf{v} \cdot \nabla \mathbf{v}) = 0 . \tag{4}$$

Here $\mathbf{v}$ with its components u and w is the wind, θ , p , and ρ are potential temperature, pressure, and density in the same order, and ζ is the vorticity. Subscript zero refers to the basic state, and quantities without subscript are the perturbation quantities. By introducing the stream function ψ , defined by

$$u = -\frac{\partial \psi}{\partial z} , \qquad w = \frac{\partial \psi}{\partial x} , \tag{5}$$

eq. (1) is automatically satisfied, and the vorticity ζ is thereby given by

$$\zeta = \nabla^2 \psi . \tag{6}$$

Elimination of $\mathbf{v}$ and ζ from (2), (3), and (4) by means of (5) and (6) leaves as unknowns ψ , θ , and p satisfying (2), (3), and (4) with appropriate boundary conditions to be imposed.

It follows from the rigid wall and symmetry assumptions that one of the boundary conditions is that of vanishing normal component of the wind along the walls and the plane of symmetry. An additional condition is that the pressure p must be exactly hydrostatic at the upper and lower boundaries and its normal derivative must be zero along the lateral boundaries. Expressed mathematically, these conditions are

$$\begin{aligned} z &= 0, H \qquad \psi = \zeta = 0 \qquad \frac{\partial p}{\partial z} = \frac{g\rho_0}{\theta_0}\theta , \\ x &= 0, L \qquad \psi = \zeta = 0 \qquad \frac{\partial p}{\partial x} = 0 . \end{aligned} \tag{7}$$

These boundary conditions determine ψ and θ uniquely, whereas p , as determined by (4) and the boundary conditions (7) contains an arbitrary constant. While θ and ψ must be given at initial time, p may not be prescribed initially but has to be determined from (4), which is a truly diagnostic equation.

Finite difference methods are used to determine numerical approximations to the solutions of the governing equations for the vorticity, stream function, pressure, and potential temperature. The scheme used for the prediction of vorticity is an explicit, three level, marching type formula essentially given by Lilly [7]. Both the stream function and the pressure are approximated by solving boundary value problems by the method of successive over-relaxation. The two step Lax-Wendroff scheme [8] is used for the hyperbolic equation for potential temperature. The mesh used is 100 m in both vertical and horizontal directions, and the time step is the smaller of 15 seconds or that determined by the stability criteria.

3. DISCUSSION OF THE RESULTS

The numerical simulation of the moist convection is being programmed for a high-speed computer and is partly checked out. Since the program is quite complex, it is necessary to check it out in suitable stages. This makes program errors easier to detect and, equally important, it facilitates the physical insight into the various stages of cloud development. The results about to be discussed pertain to shallow (3 km high) dry convection of the roll-type in neutral surroundings. Figure 1 shows the right half of the initial potential temperature bubble in °C (recall the symmetry condition), and Fig. 2 the associated pressure field in units of dyne/cm^2 , satisfying eq. (4) and the boundary condition (7). At this time, there is no circulation, and the pressure field is therefore the direct response to the temperature perturbation above. The pressure gradient is such that an inflow is set up along the lower boundary and an outflow above the core of the warm bubble with the strongest pressure gradient in its center. Note that the pressure gradient opposes the rise of the bubble which thereby experiences a form drag similar to that of a body moving through a viscous fluid.

Figure 3 shows the warm bubble and the associated circulation ($\psi \cdot 10^{6}$ in cm^2/sec) ten minutes later. The former has risen at the average rate of 0.8 m/sec during the period and the maximum vertical motion has by now reached almost 3 m/sec. Notice the mushroom shape of the bubble and the concentration of isotherms in its upper part. The associated pressure field is shown in Fig. 4. Its increased strength and change in pattern is the result of the motion field (last term in (4)) which contributes much more than the second term (direct contribution of the temperature field), a condition noticeable already in the very first minutes of the development. The main contributions of the pressure field are

a) acceleration of the inflow along the lower boundary,
b) acceleration of the inflowing air upwards along the z-axis towards the base of the bubble, an area where the buoyancy is greatly reduced or non-existant,
c) deceleration of the upper part of the rising bubble (form drag), and
d) deceleration of the outflow associated with the upper half of the bubble.

Figure 5 shows the potential temperature and stream function fields after 20 minutes, at which time the temperature maximum, considerably reduced through truncation caused by the finite difference approximation, is approaching the upper boundary. The average rate of ascent in the core over the preceding 10 minutes was 1.9 m sec which is more than twice the ascent during the first 10 minute period. The pressure field, shown in Fig. 6, is more intense than 10 minutes earlier and has a minimum of 1 mb at the point of the maximum potential temperature. Otherwise, the main features are as 10 minutes earlier.

4. PLANNED WORK

A computer program for moist convection is being written and will be checked out in two stages. The first stage will preclude the precipitation phase, and the results of cloud formation alone will be studied. Later, precipitation together with the effect of evaporation of falling rain will be included. At some point, the assumption of neutral surroundings will be dropped and more realistic surroundings introduced.

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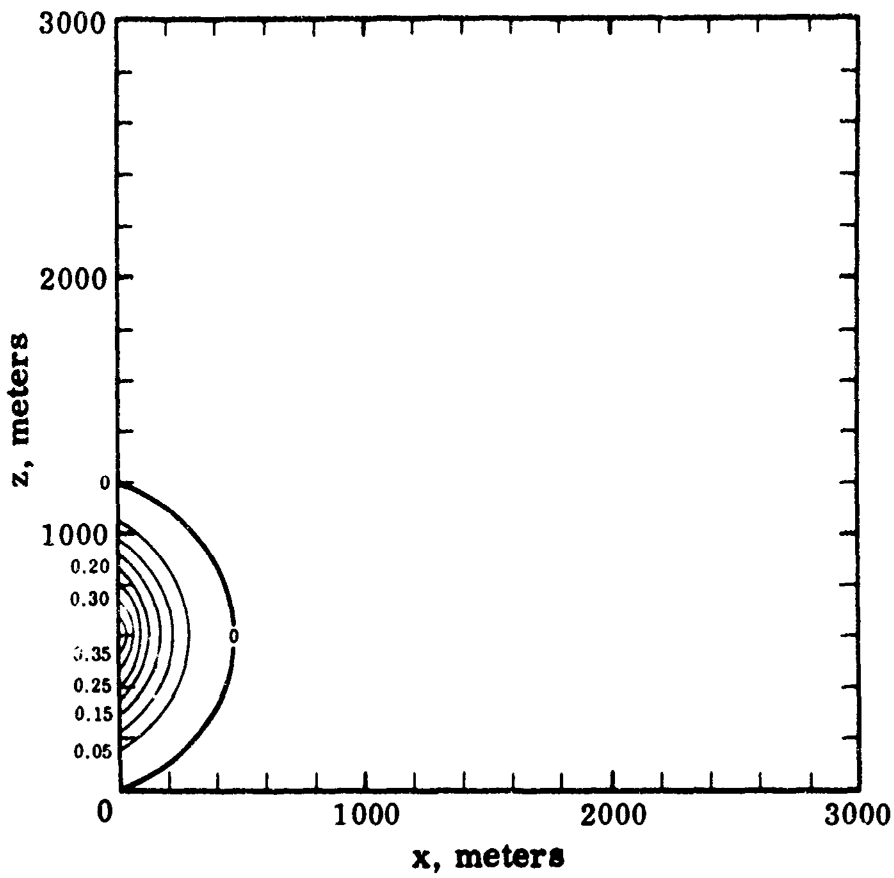

Fig. 1 Temperature, 0 minutes.

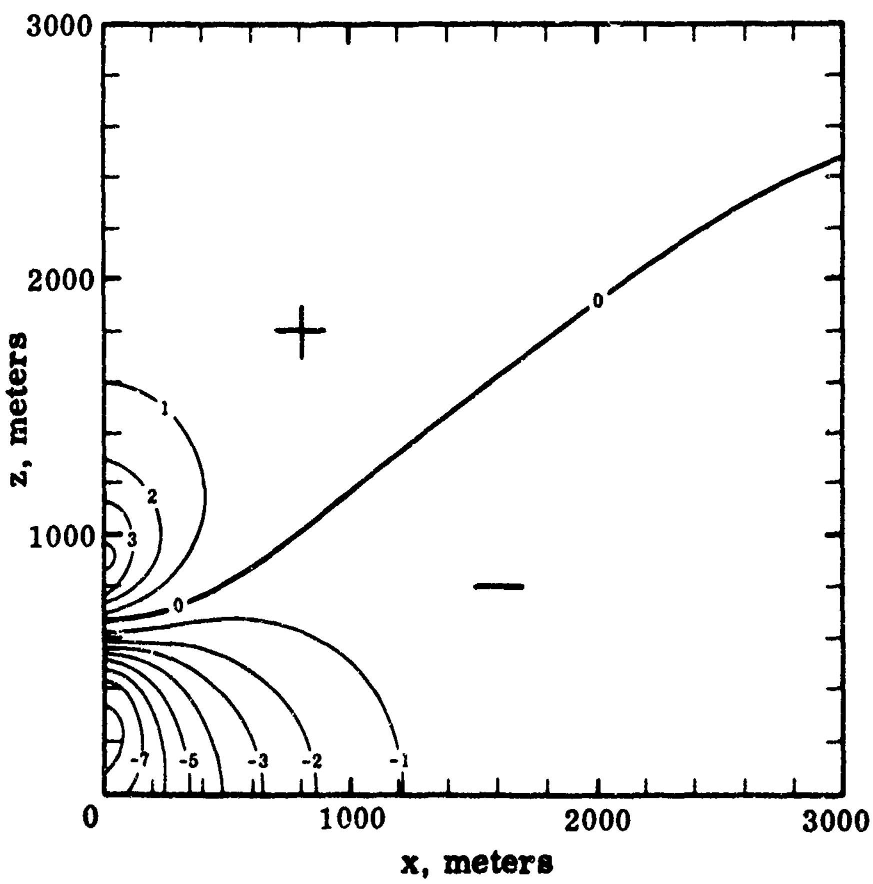

Fig. 2 Pressure, 0 minutes.

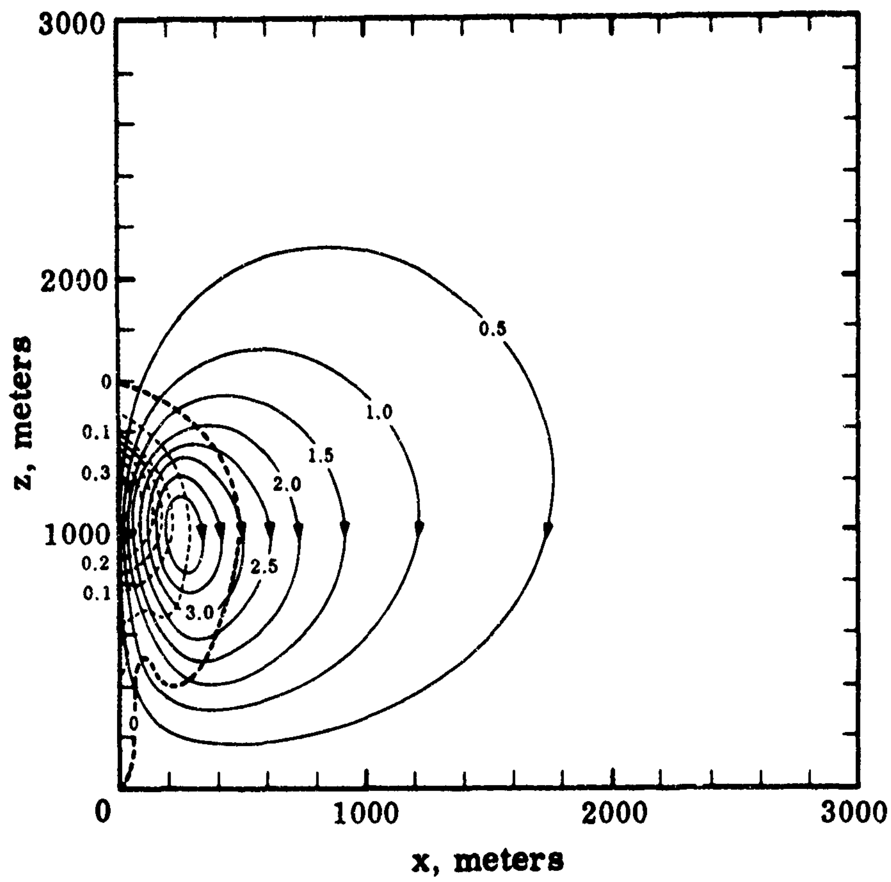

Fig. 3 Temperature and stream function, 10 minutes.

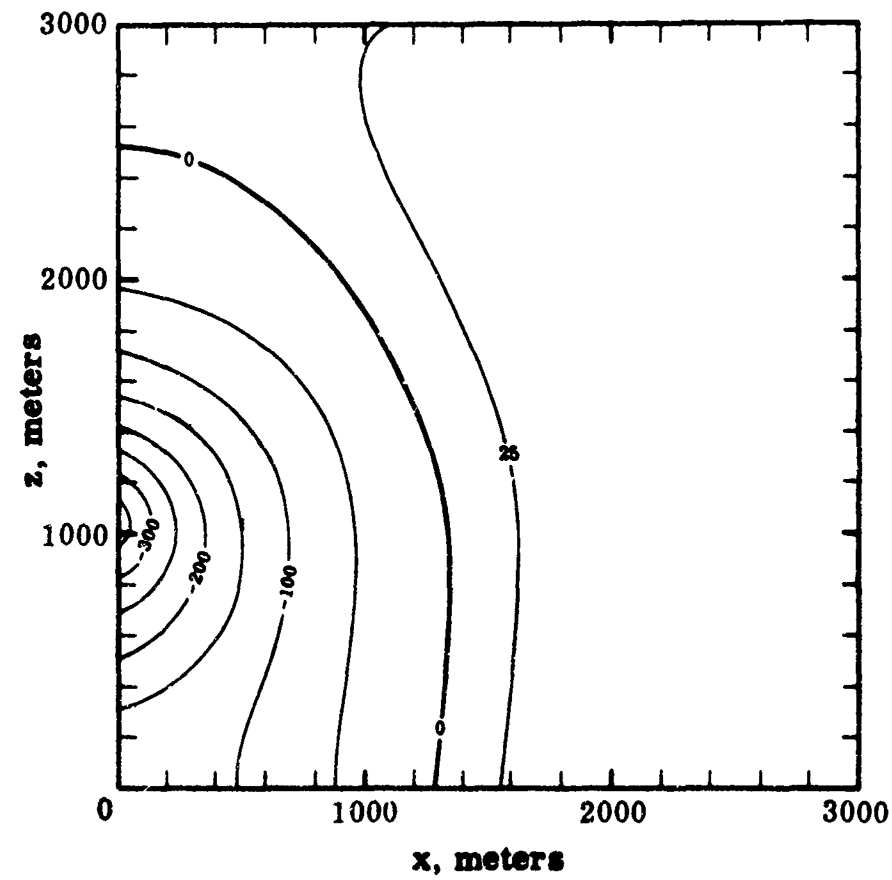

Fig. 4 Pressure, 10 minutes.

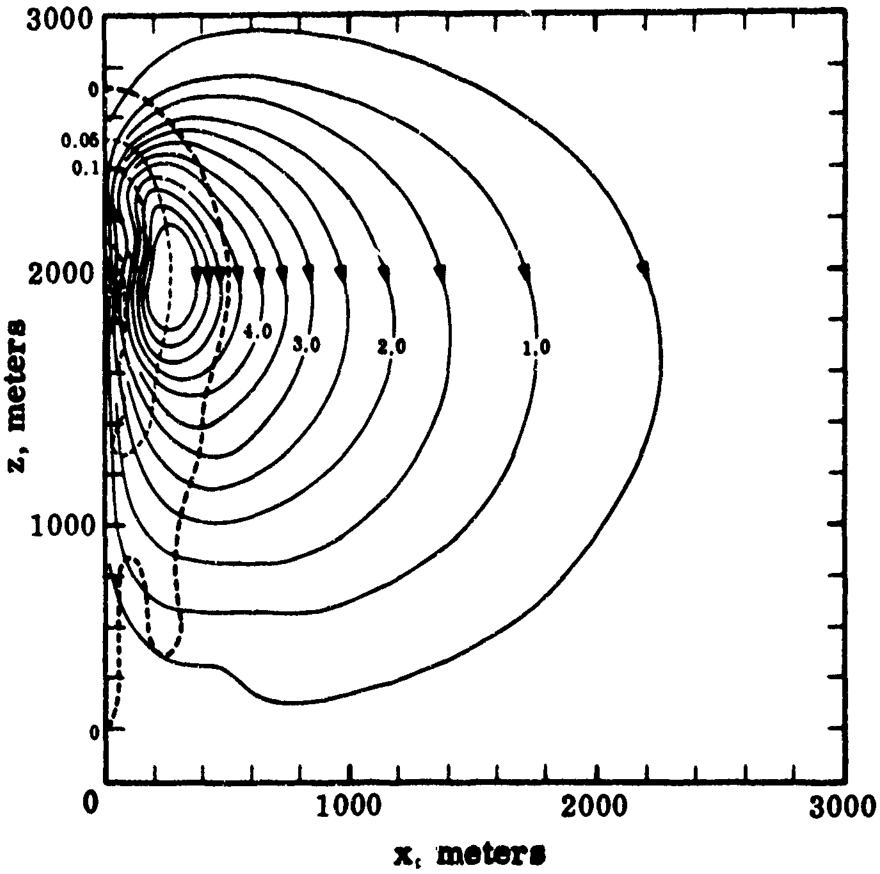

Fig. 5 Temperature and stream function, 20 minutes.

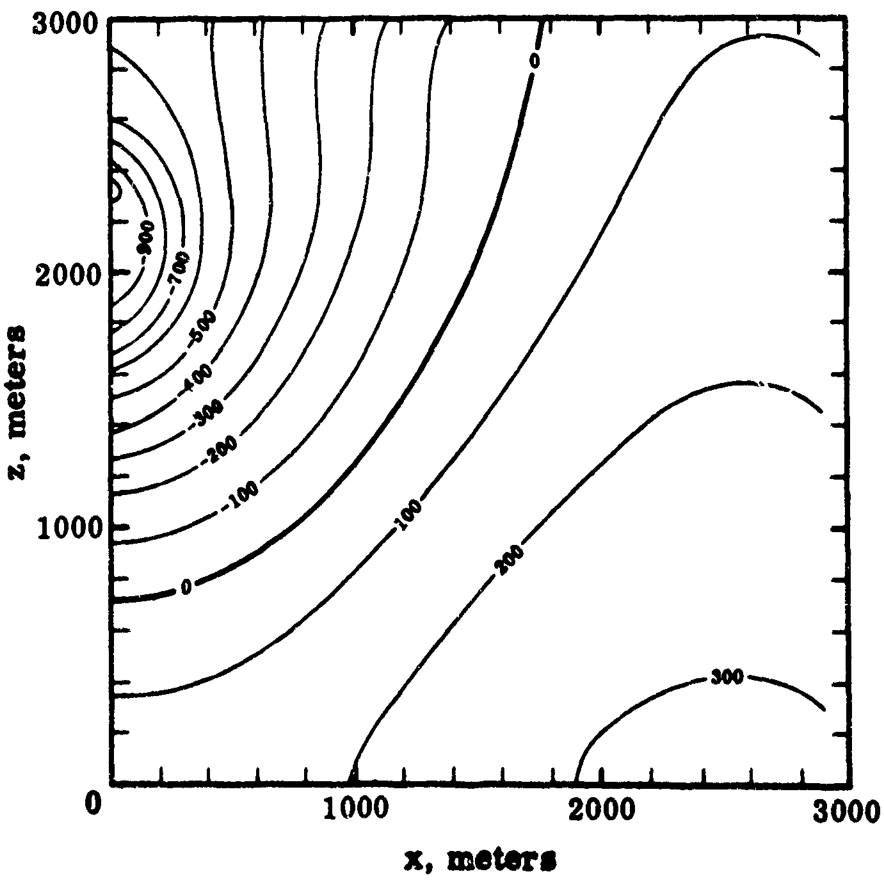

Fig. 6 Pressure, 20 minutes.

DISCUSSION ON ARNASON'S PAPER

ROSENTHAL: Would you care to say anything about the differencing scheme used here?

ARNASON: Yes, I really skipped it, but I would be happy to mention it. In serving the vorticity equation we have on the board, we used essentially the scheme described by Lilly in an article which I believe was published in the Journal of Atmospheric Sciences, which you may know, and for the thermodynamic equation we used a scheme by Lax-Wendorf, which turned out to be quite suitable so far. I will also add that we don't know whether these are the optimum schemes to use, but it was rather important to pick schemes that we had all reason to believe did work. We weren't really prepared to experiment at that stage with different schemes.

ROSENTHAL: Well, the point that I'm driving at, I guess, is that since you didn't include any dissipation in the differential equations, that you perhaps would want to use a dissipating differencing scheme. Lax-Wendorf's is, Lilly's is not, on the other hand.

ARNASON: Yes, we have no friction, simply because we knew that at least the Lax-Wendorf scheme is dissipating and we thought we would foresee just how dissipating it was before we bothered to take in the additional frictional terms.

ESTOQUE: I wonder if I could make a comment on the first slide. Isn't that the initial pressure distribution?

ARNASON: The very first slide is the temperature, but the second slide is pressure. You would like to see it?

ESTOQUE: Yes, please. There seems to be something unrealistic just from my own intuition. As you remember the initial temperature distribution was a hot blob at the center and there was no motion initially so I would have expected the pressure field to be exactly hydrostatic.

ARNASON: Why do you expect a hydrostatic pressure?

ESTOQUE: No motion.

ARNASON: Yes, but there is a forcing function.

ESTOQUE: Well, you say this arises from buoyancy, your temperature field.

FREEMAN: You already have the acceleration taken into account. That is the thing that confuses him. How can you have any pressure difference outside your region of temperature difference. You now have an acceleration field but no velocity field.

ARNASON: That's the point. There is no motion, but there is acceleration, and so we get a pressure field that is far from being hydrostatic - in fact, there's no resemblance. And, of course, it diffuses out because of your solving La Placian type equations, although the forcing function is confined to a rather small area, the pressure itself will not show us zero ---(not discernible)---.

ESTOQUE: Of course, this might be an effect of your boundary conditions also in the pressure.

ARNASON: I think they come in any kind of illustration of this type, but I think they come in in a very mild way. The pattern you have here you will get anyway. If you looked at this term you can see that the forcing function is confined to this area. It's bound to show up even if you relax the most characteristic part of the field

SOME PRELIMINARY THEORETICAL CONSIDERATIONS

OF TROPOSPHERIC WAVE MOTIONS IN EQUATORIAL LATITUDES

by
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ABSTRACT

Wave solutions to the linearized, quasi-hydrostatic equations for adiabatic, nonviscous flow on an equatorially oriented beta plane are obtained. The basic current is assumed to be zonal and invariant in both space and time. Only solutions for which the meridional-wind component is symmetric with respect to the equator are considered. Disturbances with wave lengths on the order of 10^6m are found to be very nearly nondivergent which agrees with the results of Charney's scale analysis of low-latitude atmospheric motions. The solutions show the meridional-wind component to be very nearly geostrophic even at very low latitudes. The perturbation of the zonal wind, however, is highly ageostrophic at the very low latitudes and significantly ageostrophic even in subtropical latitudes.

1. INTRODUCTION

Charney's scale analysis [3] predicts that nonviscous, adiabatic, synoptic-scale atmospheric motions near the equator should be very nearly nondivergent and, hence, that the flow should be adequately described by the conservation of absolute vorticity. The research reported on here considers the same type of motion. However, in the place of scale analysis, we examine certain wave solutions obtained from the linearized, quasi-hydrostatic equations for adiabatic, nonviscous flow on a beta plane.

Aside from the assumptions cited above, the basic current is taken to be zonal and invariant in both space and time. The discussion, therefore, pertains only to existing perturbations and provides no information concerning the manner in which they originate. It is not claimed that the theory is applicable to observed equatorial wave motions such as those described by Palmer [6]. It is quite likely that the latter are significantly affected by the release of latent heat and by quasi-barotropic instabilities due to meridional shears of the basic current. However, as will be seen later, there is a fairly reasonable superficial resemblance between the structure of the observed and theoretical waves.

Previous theoretical studies of disturbances in equatorial latitudes have assumed the flow to be nondivergent [10] or have simplified the problem through specification of one of the velocity components [7, 11]. Freeman and Graves [4] make both of these simplifications. Neither is included in the model developed below. On the other hand, Rosenthal [7] and Sherman [11] allowed the basic current to vary with latitude; this is not done here.

2. SOLUTION OF THE PERTURBATION EQUATIONS

The equations which govern nonviscous, adiabatic, quasi-hydrostatic, beta plane flow are,

$$\frac{\partial u^*}{\partial t} + u^*\frac{\partial u^*}{\partial x} + v^*\frac{\partial u^*}{\partial y} + \omega^*\frac{\partial u^*}{\partial p} - \beta y v^* + \frac{\partial \phi^*}{\partial x} = 0, \qquad (1)$$

$$\frac{\partial v^*}{\partial t} + u^*\frac{\partial v^*}{\partial x} + v^*\frac{\partial v^*}{\partial y} + \omega^*\frac{\partial v^*}{\partial p} + \beta y u^* + \frac{\partial \phi^*}{\partial y} = 0, \qquad (2)$$

$$\frac{\partial^2 \phi^*}{\partial p \partial t} + u^*\frac{\partial^2 \phi^*}{\partial p \partial x} + v^*\frac{\partial^2 \phi^*}{\partial p \partial y} + \sigma^*\omega^* = 0, \qquad (3)$$

and

$$\frac{\partial \omega^*}{\partial p} = -\left(\frac{\partial u^*}{\partial x} + \frac{\partial v^*}{\partial y}\right). \qquad (4)$$

Here, t is time, x is zonal distance, y is meridional distance measured positive northward from the equator, p is pressure, u^* is the zonal-wind component, v^* is the meridional-wind component, ω^* is the p-system vertical motion, ϕ^* is the geopotential of isobaric surfaces,

$$\sigma^* = \frac{\partial^2 \phi^*}{\partial p^2} + \left(\frac{\omega}{c_p}\right)\left(\frac{1}{p}\right)\frac{\partial \phi^*}{\partial p}$$

is the static stability, $\beta = \frac{\partial f}{\partial y}$ = a constant and f is the Coriolis parameter.

The dependent variables are written,

$$u^* = U + u(x, y, p, t), \quad U = \text{a constant}, \tag{5}$$

$$v^* = v(x, y, p, t), \tag{6}$$

$$\omega^* = \omega(x, y, p, t), \tag{7}$$

and

$$\phi^* = \bar{\Phi}(y, p) + \phi(x, y, p, t). \tag{8}$$

The variables, u, v, ω, ϕ are perturbation quantities. Since the base state must satisfy the governing equations, we find

$$\frac{\partial \bar{\Phi}}{\partial y} = -\beta y U \tag{9}$$

and, upon integration,

$$\bar{\Phi}(y, p) = \bar{\Phi}(0, p) - \frac{\beta y^2 U}{2}. \tag{10}$$

Substitution of (5), (6), (7) and (8) into (1), (2), (3) and (4), utilization of (9) and (10) yields, after linearization by the usual technique,

$$\frac{\partial u}{\partial t} + U\frac{\partial u}{\partial x} - \beta y v + \frac{\partial \phi}{\partial x} = 0, \tag{11}$$

$$\frac{\partial v}{\partial t} + U\frac{\partial v}{\partial x} + \beta y u + \frac{\partial \phi}{\partial y} = 0, \tag{12}$$

$$\frac{\partial^2 \phi}{\partial p \partial t} + U\frac{\partial^2 \phi}{\partial p \partial x} + \bar{\sigma}\,\omega = 0, \tag{13}$$

and

$$\frac{\partial \omega}{\partial p} = -\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) \tag{14}$$

where

$$\bar{\sigma} = \frac{\partial^2 \bar{\Phi}}{\partial p^2} + \left(\frac{\omega}{c_p}\right)\left(\frac{1}{p}\right)\frac{\partial \bar{\Phi}}{\partial p} \tag{15}$$

is assumed constant. The system, (11), (12), (13) and (14) has solutions of the form

$$u = A(y)\,\mathrm{Sin}\,k(x-ct)\,\mathrm{Cos}\,m(p-p_0), \tag{16}$$

$$v = B(y)\,\mathrm{Cos}\,k(x-ct)\,\mathrm{Cos}\,m(p-p_0), \tag{17}$$

$$\phi = H(y)\,\mathrm{Sin}\,k(x-ct)\,\mathrm{Cos}\,m(p-p_0), \tag{18}$$

$$\omega = W(y)\,\mathrm{Cos}\,k(x-ct)\,\mathrm{Sin}\,m(p-p_0) \tag{19}$$

where P_0 = 1000 mb, $k = \frac{2\pi}{L}$, L is the wave length, C is the wave speed and

$$m = \frac{n\pi}{p_0}. \tag{20}$$

n is an integer equal to the number of surfaces of nondivergence. From (19) and (20), $\omega = o$ at $p = o$ and $p = p_o$. Substitution of (16), (17), (18) and (19) into (11), (12), (13) and (14) gives

$$k\Delta A - \beta y B + k H = 0, \tag{21}$$

$$-k\Delta B + \beta y A + \frac{dH}{dy} = 0, \tag{22}$$

$$-mk\Delta H + \bar{\sigma} W = 0, \tag{23}$$

and

$$mW = -\left(kA + \frac{dB}{dy}\right) \tag{24}$$

where

$$\Delta = U - c \, . \tag{25}$$

When W is eliminated between (23) and (24), we obtain

$$\bar{\sigma} kA + \bar{\sigma}\frac{dB}{dy} + m^2 k\Delta H = 0. \tag{26}$$

Next, equation (21) is used to eliminate H from (22) and (26). The resulting equations are,

$$k(\bar{\sigma} - m^2\Delta^2)A + m^2\beta y\Delta B + \bar{\sigma}\frac{dB}{dy} = 0, \tag{27}$$

and

$$\beta y A - \Delta \frac{dA}{dy} + \left(\frac{\beta}{k} - k\Delta\right) B + \frac{\beta y}{k} \frac{dB}{dy} = 0. \qquad (28)$$

Finally, A may be eliminated between (27) and (28) to yield,

$$\frac{d^2B}{dy^2} - \beta^2 \left[\frac{m^2 y^2}{\sigma} - \frac{m^2\Delta}{\beta\sigma} + \frac{(\beta - k^2\Delta)(m^2\Delta^2 - \sigma)}{\sigma\beta^2\Delta}\right] B = 0, \qquad (29)$$

The following new variables are introduced,

$$\lambda = \frac{\beta y^2}{\gamma}, \qquad (30)$$

$$Q = e^{\frac{\lambda}{2}} B \qquad (31)$$

where

$$\gamma = \frac{\sigma^{-\frac{1}{2}}}{m}. \qquad (32)$$

From (29), (30), (31) and (32), we obtain

$$\lambda \frac{d^2Q}{d\lambda^2} + \left(\frac{1}{2} - \lambda\right) \frac{dQ}{d\lambda} + \frac{\alpha}{2} Q = 0 \qquad (33)$$

where

$$\frac{\gamma\left[\beta - k^2\Delta\left(1 - \frac{\Delta^2}{\gamma^2}\right)\right] - \Delta\beta}{2\Delta\beta} = \alpha \qquad (34)$$

Equation (33) is a special case of the confluent hypergeometric equation [5, p. 96]

$$s \frac{d^2T}{ds^2} + (b - s)\frac{dT}{ds} - aT = 0$$

whose general solution may be written

$$T = K_1 M(a, b, s) + K_2\, s^{1-b} M(a-b+1, 2-b, s)$$

where

$$M(a,b,s) = 1 + \frac{a}{b}\cdot\frac{s}{1!} + \frac{a(a+1)}{b(b+1)}\cdot\frac{s^2}{2!} + \text{------} .$$

The general solution to (33) is then

$$\varphi = K_1 M\left(-\frac{\alpha}{2}, \frac{1}{2}, \lambda\right) + K_2 \lambda^{\frac{1}{2}} M\left(-\frac{\alpha}{2}+\frac{1}{2}, \frac{3}{2}, \lambda\right). \tag{35}$$

By use of (30) and (31), equation (35) may be arranged to read

$$B = K_1 e^{-\frac{\beta y^2}{2\gamma}} M\left(-\frac{\alpha}{2}, \frac{1}{2}, \frac{\beta y^2}{\gamma}\right) + K_2 \left(\frac{\beta}{\gamma}\right)^{\frac{1}{2}} e^{-\frac{\beta y^2}{2\gamma}} M\left(-\frac{\alpha}{2}+\frac{1}{2}, \frac{3}{2}, \frac{\beta y^2}{\gamma}\right). \tag{36}$$

The parameter α is an eigenvalue of the problem and must be determined from side conditions imposed upon the motion. If we restrict ourselves to flows in which v is symmetric about the equator (this is the case, for example, with the Palmer waves), then $K_2 = 0$. If it is further required that v vanish at

$$y = \pm y_w \tag{37}$$

then $\alpha = \alpha_*$ where

$$M\left(-\frac{\alpha_*}{2}, \frac{1}{2}, \frac{\beta y_w^2}{\gamma}\right) = 0. \tag{38}$$

In general, numerical methods are required if (38) is to be solved for α^*

A simpler solution is obtained when the condition $v(\pm y_w) = 0$ is replaced by the less stringent restriction that v decay with distance from the equator. This can be achieved by selecting $\alpha = 0$ in which case

$$B = K_1 e^{-\frac{\beta y^2}{2\gamma}} \equiv v_0 e^{-\frac{\beta y^2}{2\gamma}} \tag{39}$$

and, from (34),

$$\left(\Delta^2 + \gamma\Delta - \frac{\gamma\beta}{k^2}\right)\left(1 - \frac{\Delta}{\gamma}\right) = 0. \tag{40}$$

The roots of (40) are

$$\Delta_1 = \gamma,^{1} \tag{41}$$

$$\Delta_2 = -\frac{\gamma}{2}\left[1 + \left(1 + \frac{4\beta}{k^2\gamma}\right)^{\frac{1}{2}}\right], \tag{42}$$

$$\Delta_3 = \frac{\gamma}{2}\left[\left(1 + \frac{4\beta}{k^2\gamma}\right)^{\frac{1}{2}} - 1\right]. \tag{43}$$

Table 1 lists some values of Δ_1, Δ_2 and Δ_3 ($\bar{\sigma}$ = [illegible] ts units; a reasonable value for the tropical mid-troposphere). Δ_1 is a pure internal gravity wave which propagates rapidly westward <u>relative</u> to the basic current. Δ_2 is an inertia-gravity wave which propagates rapidly eastward relative to the basic current. Δ_3 is also an inertia-gravity wave. However, its propagation relative to the basic current is westward at approximately the Rossby rate of $\Delta_{ND} \equiv \beta/k^2$. According to Palmer's observations [6], the motion of equatorial waves relative to the basic current is small and, therefore, we assume Δ_3 to be the meteorologically significant root.

By use of (27) and (39),

$$A = \frac{\beta y}{k(\Delta_3 + \gamma)} V_0 e^{-\frac{\beta y^2}{2\gamma}} \tag{44}$$

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[1] The derivation of equation (33) required that (27) be divided by $k(\bar{\sigma} - m^2\Delta^2) = \frac{k}{m^2}(\gamma^2 - \Delta^2)$. However, when $\Delta = \gamma$, equation (27) is encoupled and may be solved directly to yield $B = V_0 e^{-\frac{\beta y^2}{2\gamma}}$ thus showing that (39)

which shows that u is asymmetric about the equator. From (21), (39), and (44

$$H = \frac{\beta \gamma y}{k(\Delta_3 + \gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \tag{45}$$

which is also asymmetric about the equator. From (23) and (45),

$$W = \frac{\beta \Delta_3 y}{m\gamma(\gamma + \Delta_3)} v_0 e^{-\frac{\beta y^2}{2\gamma}} . \tag{46}$$

Inspection of (23) and (24) shows that the solution for nondivergent motion can be obtained by allowing $\bar{\sigma}$ to tend toward infinity while H, Δ, k and m remain finite. In this case, (43) yields

$$\Delta_3 = \lim_{\gamma \to \infty} \left[\frac{\frac{1}{2}\left[\left(1 + \frac{4\beta}{k^2\gamma}\right)^{\frac{1}{2}} - 1\right]}{\frac{1}{\gamma}} \right] \tag{47}$$

or, if l' Hospital's rule is employed,

$$\Delta_3 = \frac{\beta}{k^2} \equiv \Delta_{ND} . \tag{48}$$

From (39), the nondivergent solution for B is

$$B = v_0 \tag{49}$$

provided that y is not allowed to approach infinity. From (44),

$$A = 0 \tag{50}$$

for nondivergent motion. From (45), the nondivergent value of H is

$$H = \frac{\beta y}{k} v_0 . \tag{51}$$

(44).

Equations (49) and (50) are exactly the assumptions employed by Rossby [8]. Furthermore, (48), (49), (50) and (51) correspond precisely to Freeman and Graves' [4] recent theory of equatorial Rossby waves.

Alternately, (48), (49), (50) and (51) may be obtained by eliminating H between (21) and (22) to form the "vorticity equation" and then imposing the restriction

$$kA + \frac{dB}{dy} = 0. \tag{52}$$

The latter constrains the motion to be nondivergent. Upon executing this sequence of operations, equation (53) is obtained.

$$\frac{d^2B}{dy^2} - \frac{(k^2\Delta - \beta)}{\Delta} B = 0,[2] \tag{53}$$

The solution for symmetrical B is

$$B = v_0 \cos \frac{(2l+1)\pi}{2y_w} y, \quad l \text{ an integer} \tag{54}$$

provided that

$$\frac{(2l+1)^2\pi^2}{4y_w^2} = \frac{k^2\Delta - \beta}{\Delta}. \tag{55}$$

If, as was done in the derivations of (39), (43), (44), (45) and (46), we require B to have no zeroes over the range of y, then y_w must approach infinity. In this case, (54) reduces to (49), (55) gives (48), (52)

[2] $\Delta = 0$ corresponds to a trivial solution in the nondivergent case.

gives (50) and (51) may be obtained from (21).

By use of (43) and (48),

$$\Delta_3 = \frac{\Delta_{ND}}{\frac{1}{2} + \left(\frac{1}{4} + \frac{\Delta_{ND}}{\gamma}\right)^{\frac{1}{2}}} \quad . \tag{56}$$

When the basic current is easterly ($E = -U > 0$),

$$C_{ND} = -E - \Delta_{ND} \tag{57}$$

and

$$C_3 = -E - \frac{\Delta_{ND}}{\frac{1}{2} + \left(\frac{1}{4} + \frac{\Delta_{ND}}{\gamma}\right)^{\frac{1}{2}}} \tag{58}$$

The disturbances, therefore, propagate westward at a speed which exceeds that of the basic current but which is less than would be the case for nondivergent flow. The distribution of convergence and divergence must then be such that westward relative motion is retarded. This implies convergence to the east of zones of cyclonic relative vorticity and divergence to the west of such regions. The reverse is true of zones of anticyclonic relative vorticity and the rule is equally valid in both the Northern and Southern Hemispheres and for easterly and westerly basic currents.

From Table 1, it is clear that C_3 departs more and more from C_{ND} as the wave length increases. The model convergence and divergence is then of greater significance at the longer wave lengths. This is similar to the so-called "barotropic divergence" found in barotropic, quasi-geostrophic models which allow non-zero vertical integrals of the divergence [1, 2, 8, 9, 12 etc.].

By use of (16), (17), (18), (19), (39), (44), (45) and (46), the solutions for the dependent variables may be written

$$u = \frac{\beta y}{k(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \sin k(x-ct) \cos m(p-p_0), \tag{59}$$

$$v = v_0 e^{-\frac{\beta y^2}{2\gamma}} \cos k(x-ct) \cos m(p-p_0), \tag{60}$$

$$\phi = \frac{\beta\gamma y}{k(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \sin k(x-ct) \cos m(p-p_0), \tag{61}$$

and

$$\omega = \frac{\beta\Delta_3 y}{m\gamma(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \cos k(x-ct) \sin m(p-p_0). \tag{62}$$

3. DISCUSSION OF THE SOLUTIONS

From (14) and (62), the divergence is given by

$$\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) = D \cos k(x-ct) \cos m(p-p_0) \tag{63}$$

where

$$D = -\frac{\beta y \Delta_3}{\gamma(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \tag{64}$$

is the amplitude of the divergence. D reaches its maximum magnitude at

$$y = \pm\left(\frac{\gamma}{\beta}\right)^{\frac{1}{2}},$$

hence,

$$|\textcircled{D}|_{MAX} = \left(\frac{\beta}{\gamma e}\right)^{\frac{1}{2}} \left(\frac{\Delta_3}{\Delta_3 + \gamma}\right) v_o. \tag{65}$$

Table 2 gives $|\textcircled{D}|_{MAX}$ as a function of wave length for σ = 3mts, v_o = 5m-sec^{-1}, n = 1 and n = 2. In agreement with Charney's scale analysis [3], the values are found to be extremely small.

From (59) and (60), the relative vorticity is given by

$$\zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} = \textcircled{\zeta} \sin k(x - ct) \cos m(p - p_o) \tag{66}$$

where

$$\textcircled{\zeta} = \frac{k\left[\frac{\beta \Delta_{ND}}{\gamma} y^2 - \Delta_{ND} - \Delta_3 - \gamma\right]}{\Delta_3 + \gamma} v_o e^{-\frac{\beta y^2}{2\gamma}}. \tag{67}$$

Since $\textcircled{\zeta}$ reaches its maximum magnitude at the equator,

$$|\textcircled{\zeta}|_{MAX} = \frac{k(\Delta_{ND} + \Delta_3 + \gamma)}{\Delta_3 + \gamma} v_o. \tag{68}$$

Values of $|\textcircled{\zeta}|_{MAX}$ are given by Table 3; the ratio

$$\frac{|\textcircled{D}|_{MAX}}{|\textcircled{\zeta}|_{MAX}} = \left(\frac{\beta}{\gamma e}\right)^{\frac{1}{2}} \left[\frac{\Delta_3}{k(\Delta_{ND} + \Delta_3 + \gamma)}\right] \tag{69}$$

is shown by Table 4. We find the relative vorticity to have an order of magnitude which is meteorologically significant and which is large compared to that of the divergence. Relative to the Palmer waves [6], the model vorticity is of the correct order of magnitude. However, the Palmer waves, which typically have a wave length of about 2000 km, show divergences on the order of $10^{-6}sec^{-1}$.

In Charney's scale analysis [3], the Rossby number was assumed to be large close to the equator. For our purposes, it is convenient to define two Rossby numbers:

$$R_{ox} \equiv \left| \frac{\frac{\partial u}{\partial t} + U\frac{\partial u}{\partial x}}{\beta y v} \right| = \frac{\Delta_3}{\Delta_3 + \gamma} , \tag{70}$$

$$R_{oy} \equiv \left| \frac{\frac{\partial v}{\partial t} + U\frac{\partial v}{\partial x}}{\beta y u} \right| = \frac{\gamma}{\beta y^2} . \tag{71}$$

R_{ox} is independent of latitude while R_{oy} does not vary with wave length. From Table 5, we see that R_{ox} is quite small for the shorter wave lengths. For these wave lengths, therefore, the symmetrical (v) component of the motion is very nearly in geostrophic equilibrium even at very low latitudes. The behaviour of the asymmetrical (u)-component is quite different. R_{oy} tends to infinity as the equator is approached. Table 6 shows that the u-component is markedly ageostrophic even in subtropical latitudes. Because of the largeness of R_{oy} close to the equator, it is clear that a Rossby number for the total motion would also be large in equatorial latitudes which agrees with Charney's [3] basic assumption.

By cross differentiation of (11) and (12), the linearized vorticity equation,

$$\frac{\partial \zeta}{\partial t} + U\frac{\partial \zeta}{\partial x} + \beta v + \beta y\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) = 0 \tag{72}$$

is obtained. From (59), (60) and (72),

$$\frac{D\zeta}{Dt} + \beta v + f\nabla\cdot V = 0$$

or

$$\frac{\frac{D\zeta}{Dt}}{\beta v} + 1 + \frac{f\nabla\cdot V}{\beta v} = 0 \tag{73}$$

where $\frac{D\zeta}{Dt}$, βv and $f\nabla\cdot V$ are, respectively, the amplitudes of $\frac{\partial\zeta}{\partial t} + U\frac{\partial\zeta}{\partial x}$, βv and $\beta y\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right)$:

$$\frac{D\zeta}{Dt} = -\frac{k^2\Delta_3}{\Delta_3+\gamma}\left[\frac{\beta\Delta_{ND}}{\gamma}y^2 - \Delta_{ND} - \Delta_3 - \gamma\right] v_0 e^{-\frac{\beta y^2}{2\gamma}}, \tag{74}$$

$$\beta v = \beta v_0 e^{-\frac{\beta y^2}{2\gamma}}, \tag{75}$$

$$f\nabla\cdot V = -\frac{\beta^2 y^2\Delta_3}{\gamma(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}}. \tag{76}$$

From (70), (71), (75) and (76),

$$\left|\frac{f\nabla\cdot V}{\beta v}\right| = \frac{\beta y^2\Delta_3}{\gamma(\Delta_3+\gamma)} = \frac{R_{ox}}{R_{oy}}. \tag{77}$$

Values of $\left|\dfrac{f\nabla\cdot V}{\beta v}\right|$ for L = 2000 km, n = 1 and L = 5000 km, n = 2 are listed by Table 7. With L = 2000 km, n = 1, we find, as predicted by Charney [3], that the divergence term in the vorticity equation is negligible at very low latitudes. On the other hand, with L = 5000 km, n = 2, this term becomes significant within 5 to 10 degs. lat. of the equator.

We now turn to a description of some synoptic aspects of the theoretical disturbances. Since the motion is very nearly nondivergent, it is convenient and appropriate to introduce a stream function. From the Helmholtz theorem,

$$u = -\frac{\partial \psi}{\partial y} + \frac{\partial \chi}{\partial x} \tag{78}$$

and

$$v = \frac{\partial \psi}{\partial x} + \frac{\partial \chi}{\partial y} \tag{79}$$

where ψ is the stream function for the perturbation motion and χ is the corresponding velocity potential. If we neglect the potential flow and make use of (59) and (60), (78) becomes

$$\frac{\partial \psi}{\partial y} \approx -\frac{\beta y}{k(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \sin k(x-ct) \cos m(p-p_0) \tag{80}$$

and (79) yields

$$\frac{\partial \psi}{\partial x} \approx v_0 e^{-\frac{\beta y^2}{2\gamma}} \cos k(x-ct) \cos m(p-p_0). \tag{81}$$

Integration of (80) gives

$$\psi \approx \frac{\gamma}{k(\Delta_3+\gamma)} v_0 e^{-\frac{\beta y^2}{2\gamma}} \sin k(x-ct) \cos m(p-p_0) + \psi_1(x,p,t) \tag{82}$$

where $\psi_1(x, p, t)$ is an arbitrary function. The integral of (81) is

$$\psi \approx \frac{v_0}{k} e^{-\frac{\beta y^2}{2\gamma}} \sin k(x-ct) \cos m(p-p_0) + \psi_2(y, p, t) \quad (83)$$

where $\psi_2(y, p, t)$ is also arbitrary. If ψ_1 and ψ_2 are suppressed, the expressions for ψ given by (82) and (83) are approximately equal since $\frac{\gamma}{\Delta_3+\gamma} \approx \frac{\gamma}{\gamma} = 1$. Probably a better estimate of ψ than either (82) or (83) is given by their average.

$$\psi \approx \frac{1}{2}\left[\frac{\gamma}{\Delta_3+\gamma} + 1\right] \frac{v_0}{k} e^{-\frac{\beta y^2}{2\gamma}} \sin k(x-ct) \cos m(p-p_0). \quad (84)$$

Figure 1 shows the ψ-pattern for $\sigma = 3$ mts, $v_0 = 5$ m-sec^{-1}, L = 2000 km, n = 1, t = o and p = 1000 mb. The motion consists of alternating, north-south elongated cells of clockwise and counterclockwise circulations each of which is centered on the equator. The clockwise cells are cyclonic in the Southern Hemisphere and anticyclonic in the Northern Hemisphere. The reverse is true of the counterclockwise cells. Relative to the field of perturbation-pressure height (computed from (61) and shown by Figure 2), cyclonic perturbation motion is associated with low pressure-height and the reverse is true of anticyclonic perturbation motion. The field of pressure-height itself is composed of alternating highs and lows centered asymmetrically at some distance from the equator. The amplitude of the pressure-height perturbation is only a few meters and, therefore, if waves of this type were to exist in the real atmosphere, the variability of pressure-height associated with them would probably go undetected. The total-stream function and pressure-height (Figures 3 and 4, respectively), which consists of the sums of the perturbation and base-state quantities (with $\bar{U} = -7.5$ m-sec^{-1}) show a close resemblance to the streamlines and pressure patterns of Palmer's [6] empirical model (see his Figure 8 and 9).

The 1000 mb-fields of perturbation-relative vorticity and divergence are shown, respectively, by Figures 5 and 6. Positive relative vorticity corresponds to counterclockwise turning and, hence, to cyclonic relative vorticity in the Northern Hemisphere and anticyclonic relative vorticity in the Southern Hemisphere. The reverse is true for negative relative vorticity. Thus, as would be expected from our comparison of the perturbation-stream function with the perturbation pressure-height, cyclonic relative vorticity is associated with low perturbation pressure-height and the reverse is true for anticyclonic relative vorticity.

Everywhere, except at the equator itself, convergence occurs to the east of cyclonic relative vorticity and divergence occurs to the east of anticyclonic relative vorticity (Figure 6). This is consistent with the distribution of convergence and divergence previously deduced from the frequency equation. The spatial distribution of convergence and divergence is in good agreement with that of Palmer's empirical model [6: Figure 10], however, the magnitudes given by the theory are far too small.

4. CONCLUSIONS

In equatorial latitudes, nonviscous, adiabatic perturbations superimposed upon an invariant basic current are very nearly nondivergent provided that the wave length is of the order of 10^6m. If the meridional-wind component is symmetric with respect to the equator, it will be very nearly geostrophic even at very low latitudes. On the other hand, the asymmetric (zonal) component of the perturbation wind will be highly ageostrophic in equatorial latitudes and significantly ageostrophic even in subtropical latitudes. The order of magnitude of the relative vorticity obtained from the theory is in agreement with that of observed equatorial disturbances as described by Palmer [6]. The order of magnitude of the divergence given by the model is quite small compared to that found empirically by Palmer [6]. The amplitude of the geopotential perturbation given by the theory is extremely small; with present standards of observation in equatorial latitudes, it would probably be undetectable. As is the case in higher latitudes, the model shows cyclonic flow to be associated with low geopotential and the reverse to be true of anticyclonic flow.

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Figure Captions

1. Perturbation-stream function computed from equation (84). Isopleths are labeled in units of 10^5 m^2 - sec^{-1}. $\bar{\sigma}$ = 3 mts, L = 2000 km, $\mathcal{V}_o$ = 5 m-sec^{-1}, t = o, p = p_o, n = 1.

2. Perturbation pressure-height computed from equation (61). Isopleths are labeled in units of meters. Parameters are the same as for Figure 1.

3. Total-stream function obtained by adding to Figure 1 the stream function for a constant easterly current of 7.5 m-sec^{-1}. Isopleths are labeled in units of 10^5 m^2 - sec^{-1}. Parameters are the same as for Figure 1.

4. Total pressure-height obtained by adding to Figure 2 the pressure-height computed from equation (10) for a basic easterly current of 7.5 m - sec^{-1}. Isopleths are labeled in units of meters. Parameters are the same as for Figure 1.

5. Relative vorticity computed from equations (66) and (67). Positive values indicate counterclockwise rotation. Isopleths are labeled in units of 10^{-6} sec^{-1}. Parameters are the same as for Figure 1.

6. Divergence computed from equations (63) and (64). Isopleths are labeled in units of 10^9 sec^{-1}. Parameters are the same as for Figure 1.

Table 1. Roots of the frequency equation (40) as given by equations (41), (42) and (43). Values are given in m-sec^{-1}. n = 1 corresponds to one surface of non-divergence; n = 2 corresponds to two surfaces of non-divergence. The static stability is taken as 3 mts units. $\Delta_{ND} = \beta / k^2$ is the value of $U - C$ appropriate to nondivergent motion.

		n = 1			n = 2		
L(Km)	Δ_{ND}	Δ_1	Δ_2	Δ_3	Δ_1	Δ_2	Δ_3
1000	0.58	55.0	-55.6	0.55	27.5	-28.2	0.55
2000	2.3	55.0	-57.2	2.2	27.5	-29.8	2.2
3000	5.2	55.0	-59.7	4.7	27.5	-32.0	4.4
4000	9.3	55.0	-63.0	8.0	27.5	-35.0	7.3
5000	14.4	55.0	-66.7	11.8	27.5	-38.2	10.5

Table 2. Values of the maximum magnitude of the divergence as computed from equation (65). $\bar{\sigma}$ = 3 mts and U_o = 5 m-sec^{-1}. Values are given in units of sec^{-1}.

L(Km)	n = 1	n = 2
1000	2.0×10^{-8}	5.6×10^{-8}
2000	7.9×10^{-8}	1.9×10^{-7}
3000	1.6×10^{-7}	3.9×10^{-7}
4000	2.5×10^{-7}	5.9×10^{-7}
5000	3.5×10^{-7}	7.9×10^{-7}

Table 3. Values of the maximum magnitude of the relative vorticity computed from equation (68). $\bar{\sigma}$ =3mts and V_0 = 5 m-sec^{-1}. Values are given in units of sec^{-1}.

L(Km)	n = 1	n = 2
1000	5.0×10^{-5}	5.0×10^{-5}
2000	2.6×10^{-5}	2.7×10^{-5}
3000	1.6×10^{-5}	1.9×10^{-5}
4000	1.4×10^{-5}	1.6×10^{-5}
5000	1.2×10^{-5}	1.4×10^{-5}

Table 4. Values of the ratio $\dfrac{|(D)|_{MAX}}{|(\zeta)|_{MAX}}$ computed from equation (69). $\bar{\sigma}$ = 3 mts.

L(Km)	n = 1	n = 2
1000	4.0×10^{-4}	1.1×10^{-3}
2000	3.0×10^{-3}	7.0×10^{-3}
3000	6.9×10^{-3}	2.0×10^{-2}
4000	1.8×10^{-2}	3.7×10^{-2}
5000	2.9×10^{-2}	5.6×10^{-2}

Table 5. Values of R_{ox} computed from equation (70).
$\bar{\sigma}$ = 3 mts units.

L(Km)	n = 1	n = 2
1000	0.01	0.02
2000	0.04	0.07
3000	0.08	0.14
4000	0.13	0.21
5000	0.18	0.28

Table 6. Values of R_{oy} computed from equation (71).
$\bar{\sigma}$ = 3 mts.

Lat(degs.)	n = 1	n = 2
1	195	97.5
2	49.1	24.5
3	21.8	10.9
4	12.2	6.10
5	7.40	3.70
10	1.95	0.98
15	0.87	0.44
20	0.44	0.22
25	0.31	0.16
30	0.22	0.11

Table 7. Values of the ratio $\left| (f\mathbf{V}\cdot\nabla) / (\partial_t) \right|$ computed from equation (77). σ = 3 mts units.

Lat(degs.)	L = 2000 Km, n = 1	L = 5000 Km, n = 2
0	0	0
5	0.005	0.07
10	0.02	0.22
15	0.05	0.54
20	0.08	1.14
25	0.13	1.73
30	0.19	2.62

Fig 1

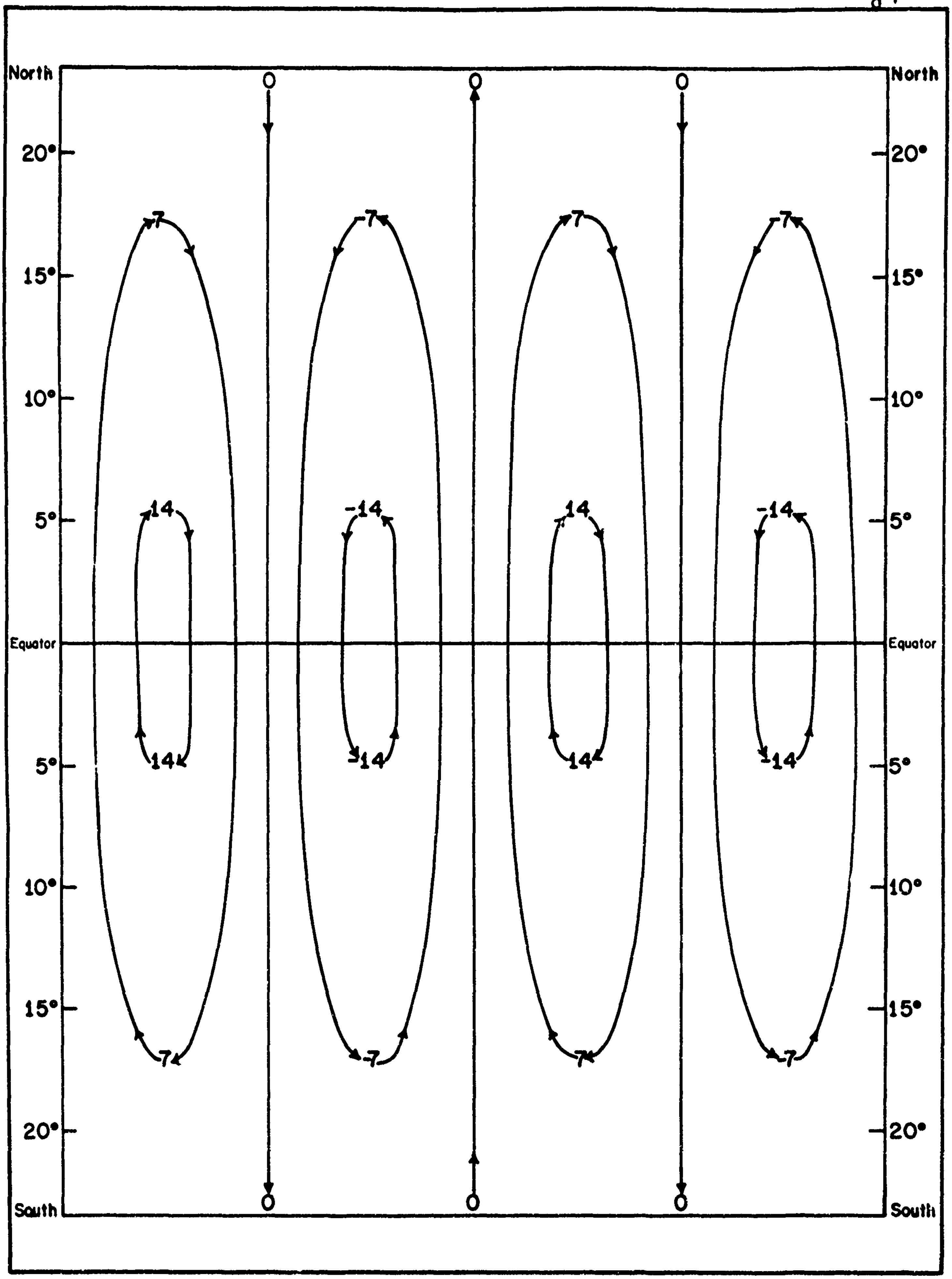
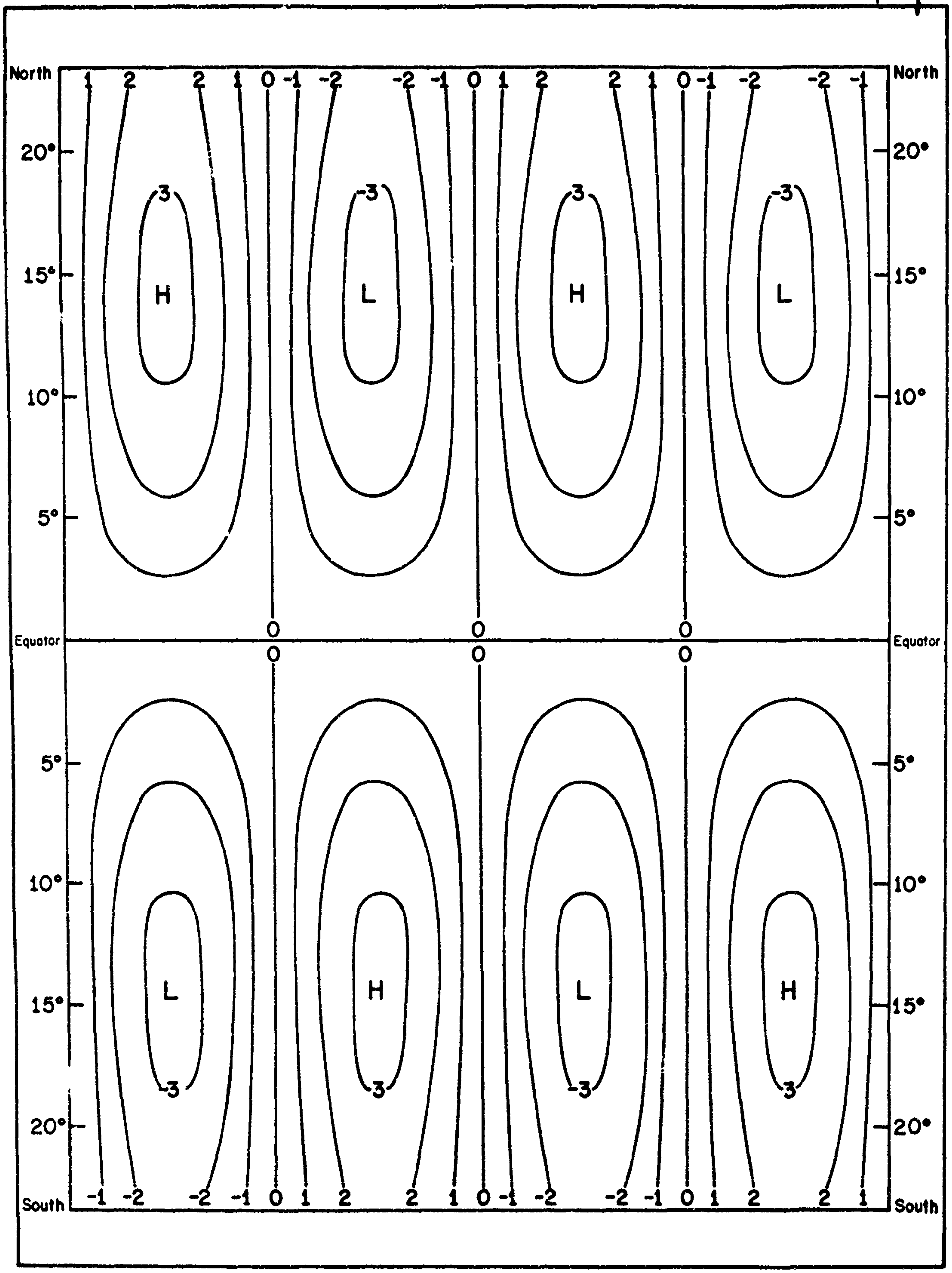


Fig 2


North
1 2 2 1 0 -1 -2 -2 -1 0 1 2 2 1 0 -1 -2 -2 -1
North
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L
Equator
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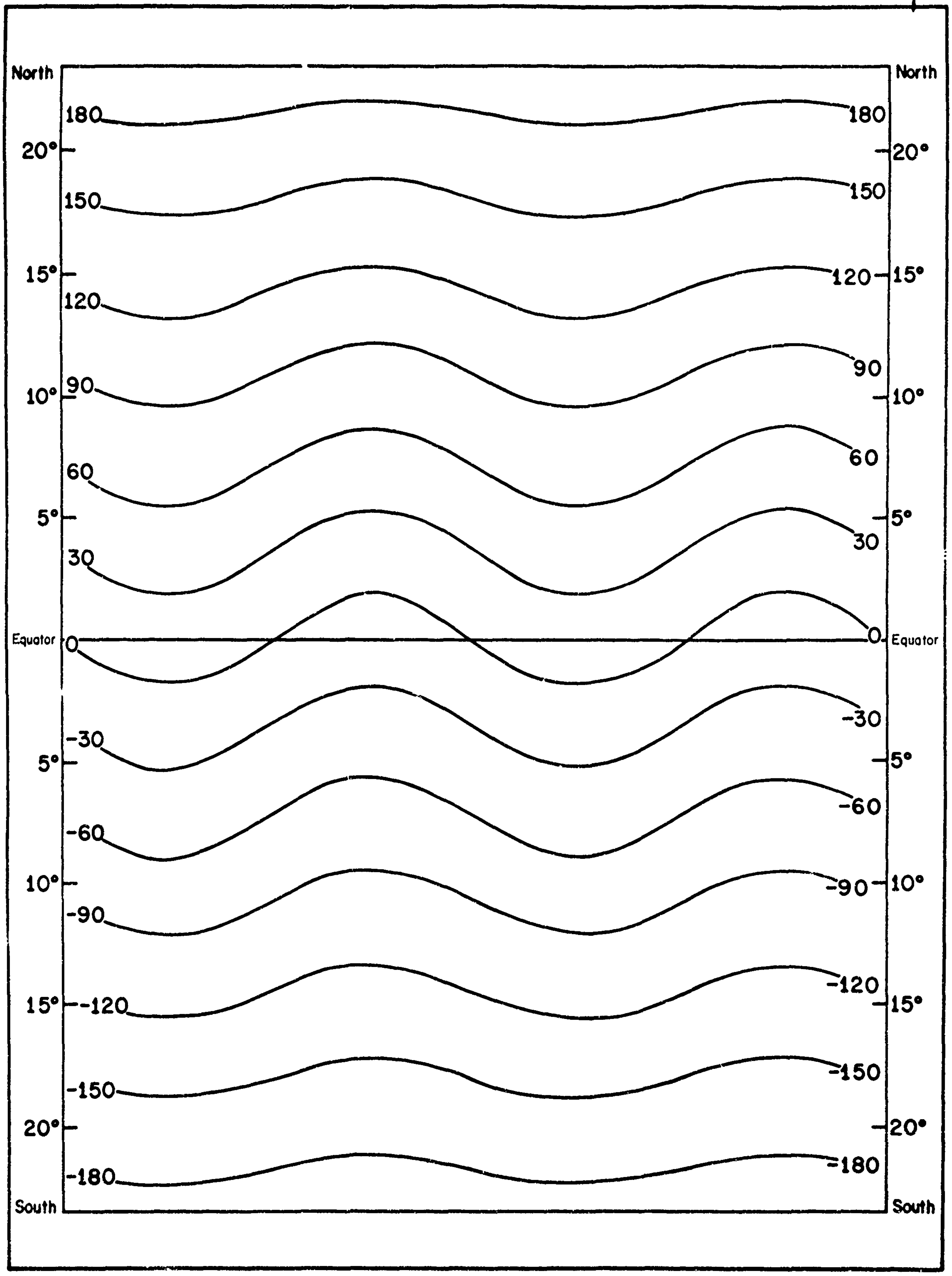
20°
South
-1 -2 -2 -1 0 1 2 2 1 0 -1 -2 -2 -1 0 1 2 2 1
South

Fig 3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20°
20°
150
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15°
120
120
15°
90
90
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10°
60
60
5°
5°
30
30
Equator
0
0
Equator
-30
-30
5°
5°
-60
-60
10°
-90
-90
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15°
-120
-120
15°
-150
-150
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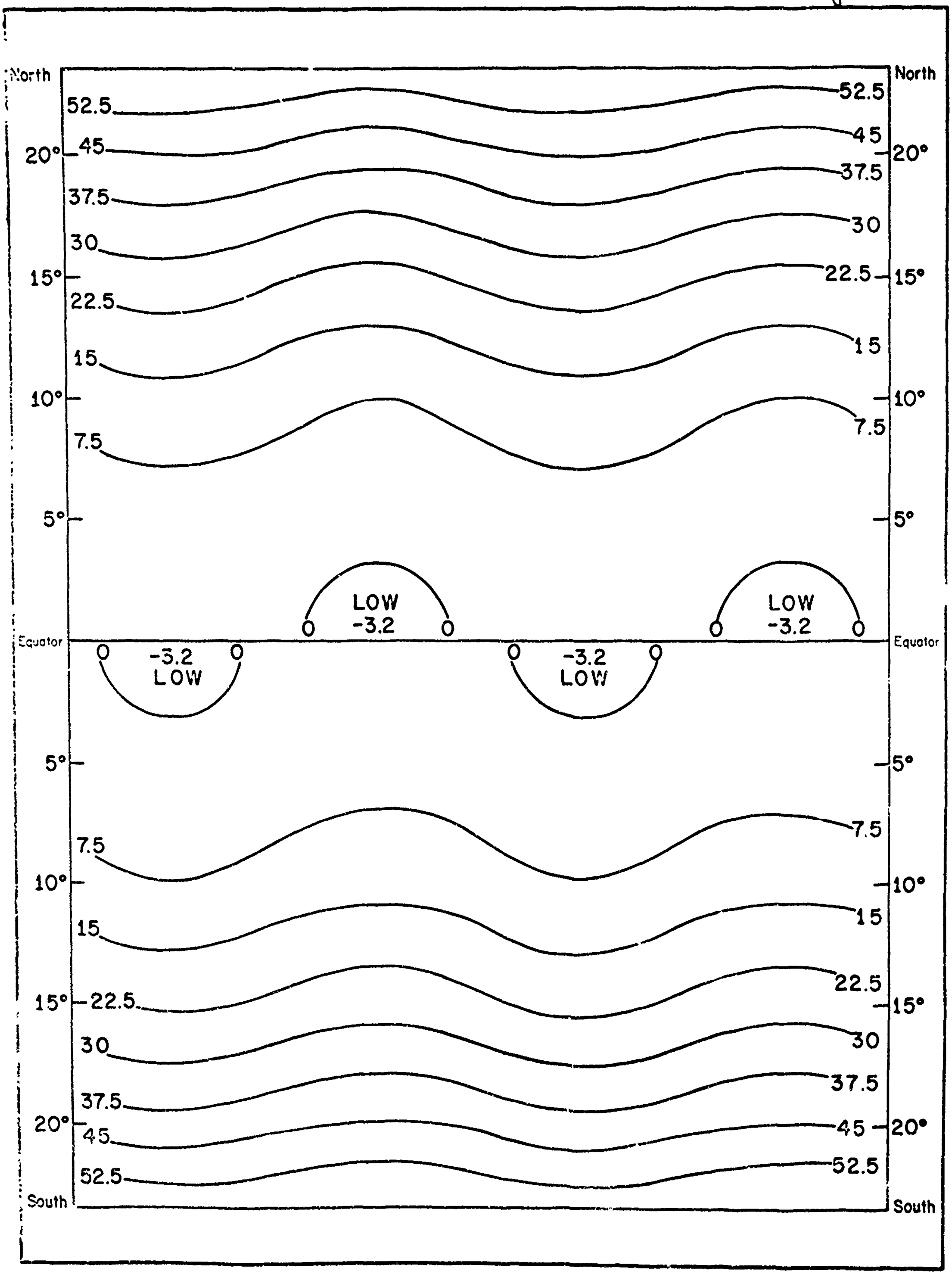
-180
-180
South
South

Fig 4


North
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45
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15
15
10°
10°
7.5
7.5
5°
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LOW
-3.2
LOW
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Equator
Equator
-3.2
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-3.2
LOW
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5°
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South

Fig 5

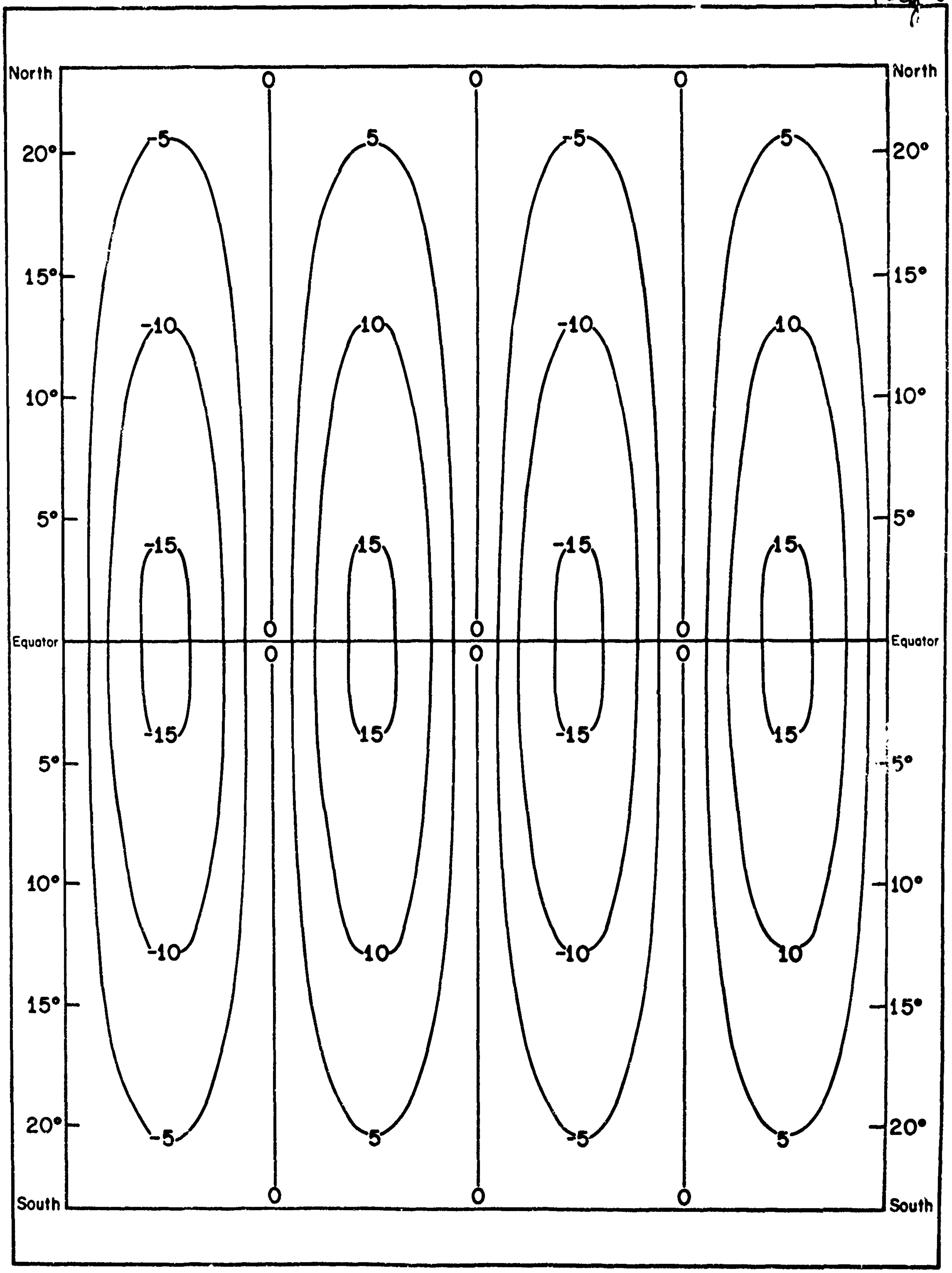
North
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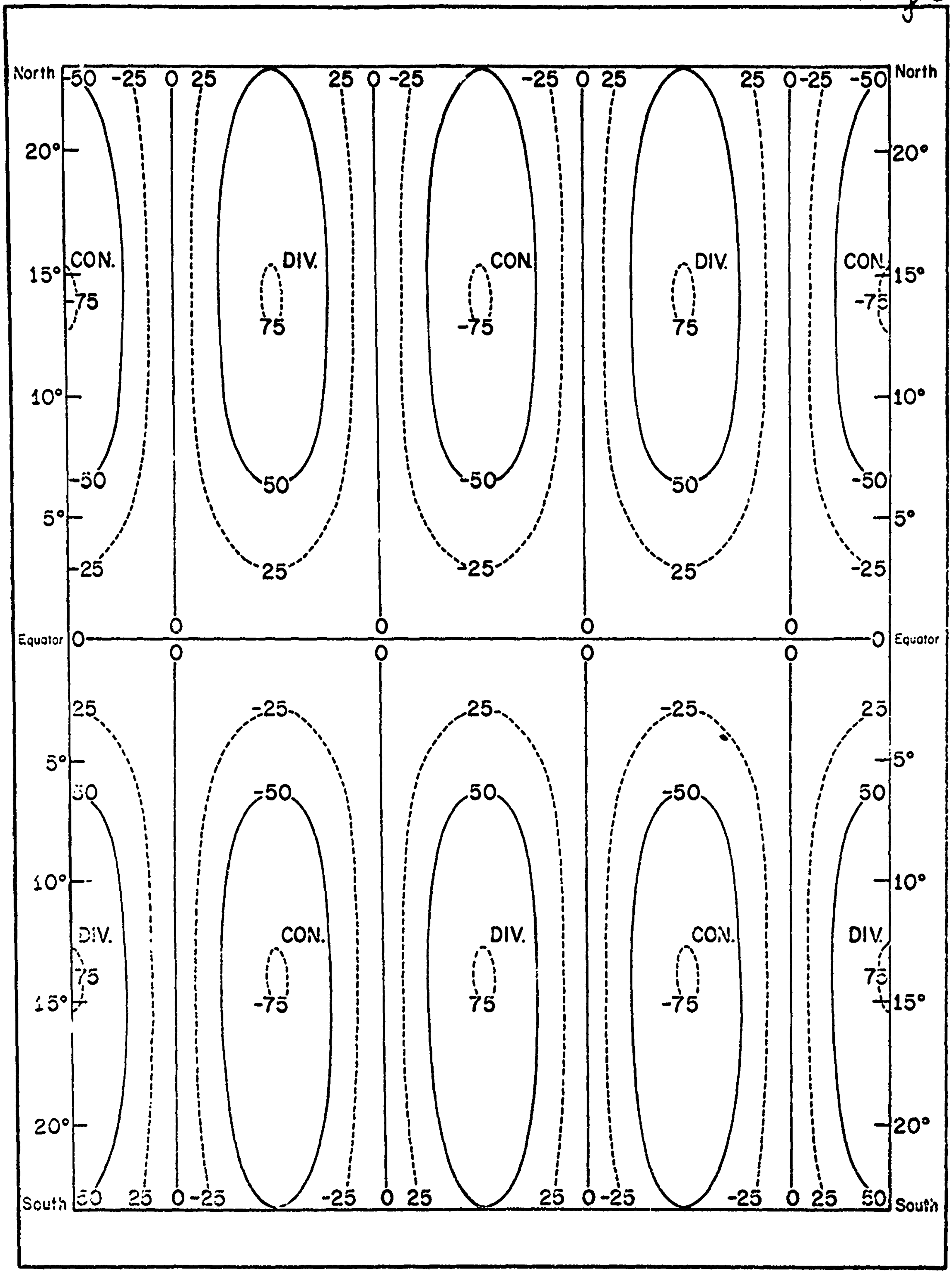
Fig 6



DISCUSSION ON ROSENTHAL'S PAPER

LA SEUR: Isn't one difference between your divergence and barotropic divergence that the barotropic divergence is independent of pressure and you have a level of non-divergence, at least one, so that your integrated divergence is zero; whereas, barotropic divergence is independent of pressure?

ROSENTHAL: Yes, in a sense, barotropic divergence is indeed independent of pressure. This, if you like, is something like an equivalent barotropic divergence.

FREEMAN: We worked with the Rossby waves----

ROSENTHAL: If I may interrupt your question a second, I would like to say that your solution falls out of this model as we allow gamma to approach infinity. That is as the static stability approaches infinity, then your solution falls out as a special case of this one.

FREEMAN: I wanted to essentailly look at those low cells, where we have the minutes marked. They show a pattern similar to the one you had. I should think they would be almost exactly the same along the equator. These are height contour lines.

ROSENTHAL: Almost, but not quite exact. The fact that your derivation suppresses the meridional variation of V and therefore interacts with the pressure field.

FREEMAN: What I wanted to know is, if you look at our zero line, it would look a lot like the lobe cell that you drew. It would be distorted, but we would have a triangle where you had a semi-circle. Now, is that the kind of height contour you were drawing as if we made a zero height contour that would be almost triangular?

ROSENTHAL: Well, mine don't slope as yours did.

FREEMAN: The thing that bothered me was the way you had your zero height contour drawn. Was there also one at the equator in your diagram?

ROSENTHAL: Yes, the equator is a zero height contour. If you recall the geo potential was proportional to Y times the exponential.

FREEMAN: All right, then you have answered my question, and that is that you had a closed zero height contour of semi-circular shape near the equator for a low cell.

ROSENTHAL: As I say, the only difference between your solution and mine is due to the presence of divergence. If you let the static stability approach infinity, mine goes to yours because the motion becomes non-divergent as was your case.

ARNASON: I have a comment on the Rossby number. It isn't really very clear how to define it because for one thing if you related it to the perturbation motion. You can get two different things depending upon its component. Second, you speak about the total flow. Maybe you should take the basic flow in, too. It's not clear.

ROSENTHAL: Yes, that's right.

ARNASON: In this case you probably would get a smaller Rossby number.

ROSENTHAL: No you would get a large Rossby number, because the piece that's due to the V component is small, the piece that is due to the basic current is also small, because there is no acceleration. In fact, that piece is zero, because the inertial part of that is, well, we have a non-linear interaction to worry about if you want to do this, I suppose. But I think the piece that is due to the U component would dominate because this goes to infinity at the equator. The U perturbation, that is.

ARNASON: I want to make myself clear, but if you go back to various kinds of solutions in fluid dynamics, usually when we define these kinds of numbers they are really defined in terms of basic quantities rather than the perturbation quantities that you would take. Say Reynolds number, you would have the basic flow, you would have the viscosity, you would have diameter, and the actual say turbulent motion doesn't even come in. What I really mean, its a tricky business.

ROSENTHAL: Oh, yes, there's no question about it, but when you use basic characteristic velocities and dimensions, what you are trying to measure is certain physical effects. When you define a Rossby number, what you are trying to measure is the ratio of the acceleration to the Coriolis terms, and this is the way I defined it, component-wise, though.

FREEMAN: Well, an indication that it's a tricky business is that if you look at your total U component of the velocity, most of it is in geostrophic balance and yet you have made the statement that the U component has the poor geostrophic balance. In other words, the base flow was in geostrophic balance, that's the largest per cent of the U component, so most of the U component was in geostrophic balance.

ROSENTHAL: Well, now, we're quickly getting into semantics.

FREEMAN: I know, but I say that this is an indication that it is tricky.

ROSENTHAL: My statement still stands and that is the ratio of the meridional acceleration to fu is large close to the equator and also large in subtropical latitudes. Now, you can call this ratio anything you please, but it is still a large ratio.

LOW-LEVEL GRAVITY WAVES IN OFF-SHORE WINDS

E. B. Kraus

Gravity waves behind mountain ranges have been studied by Scorer (1951) and others. Malkus and Stern (1954) treated the heated air over an island like amountain range and found a similar development of gravity waves behind this obstacle. The energy of the waves in both these cases is derived from the kinetic energy of the wind or of the wind shear. This means that the mean kinetic energy of the airstream has to decrease for the waves to form.

During a series of observations in the lee of the island of Aruba, we have found that gravity waves may form also close to the sea surface in a region where the air streams off the heated island over the colder sea. The motion there is accelerated. Energy for the waves there is probably derived from a decrease of the potential energy which is associated with the cooling from below.

Scorer's expression for two-dimensional gravity waves has the form:

$$\frac{\partial^2 w}{\partial z^2} + \left[\left(\frac{F}{U}\right)^2 - \frac{1}{U}\frac{\partial^2 U}{\partial z^2} - k^2\right]w = 0$$

where F is the square of the Väisälä frequency, U is the air velocity, w the perturbation vertical velocity, and k the horizontal wave number. Waves of finite energy can form only if the Väisälä frequency is real. Air which crosses a coast from a heated island or continent is likely to be warmer than the sea. The difference is further enhanced by upwelling in the water in the off-shore stream. In the case of Aruba this can result in quite large air-sea temperature differences, particularly during the daytime, as shown for example in the following table:

TABLE I

Temperatures of the sea ($z \sim -10$ cm) and of the air ($z \sim 80$ cm) as observed from drifting boat (14/3/65 at 2pm)

Dist. from beach (m)	0	50	100	150	200	250	300	350	400	450	500	550	600	650
Air temperature	29.2	27.8	27.8	27.6	28.3	27.8	27.2	27.3	27.1	27.2	27.8	27.7	27.0	27.1
Sea temperature	27.4	27.5	26.8	26.5	26.4	26.4	26.5	26.4	26.2	26.3	26.0	26.0	25.8	25.5
Δ	1.8	.3	1.0	1.1	1.9	1.4	.7	.9	.9	.9	1.8	1.7	1.2	1.6

Dist. from beach (m)	700	750	800	[illegible]	900	950	1000	1100	1150	1200
Air temperature	27.3	27.0	27.2	26.6	26.9	26.9	26.6	27.1	26.8	26.5
Sea temperature	25.3	25.0	25.0	25.0	25.1	25.2	25.2	25.2	25.2	25.1
Δ	2.0	2.0	2.2	1.6	1.8	1.7	1.4	1.9	1.6	1.4

The sea is relatively warm close in shore. As the air streams out from the beach it is quickly cooled in the lowest layers and the temperature difference becomes relatively small. Some further distance out, in the present case about 500 meters from the shore, the water temperature drops further because of the upwelling which now becomes noticeable. As a result, the vertical temperature difference increases again. This produces favorable conditions for the development of gravity waves.

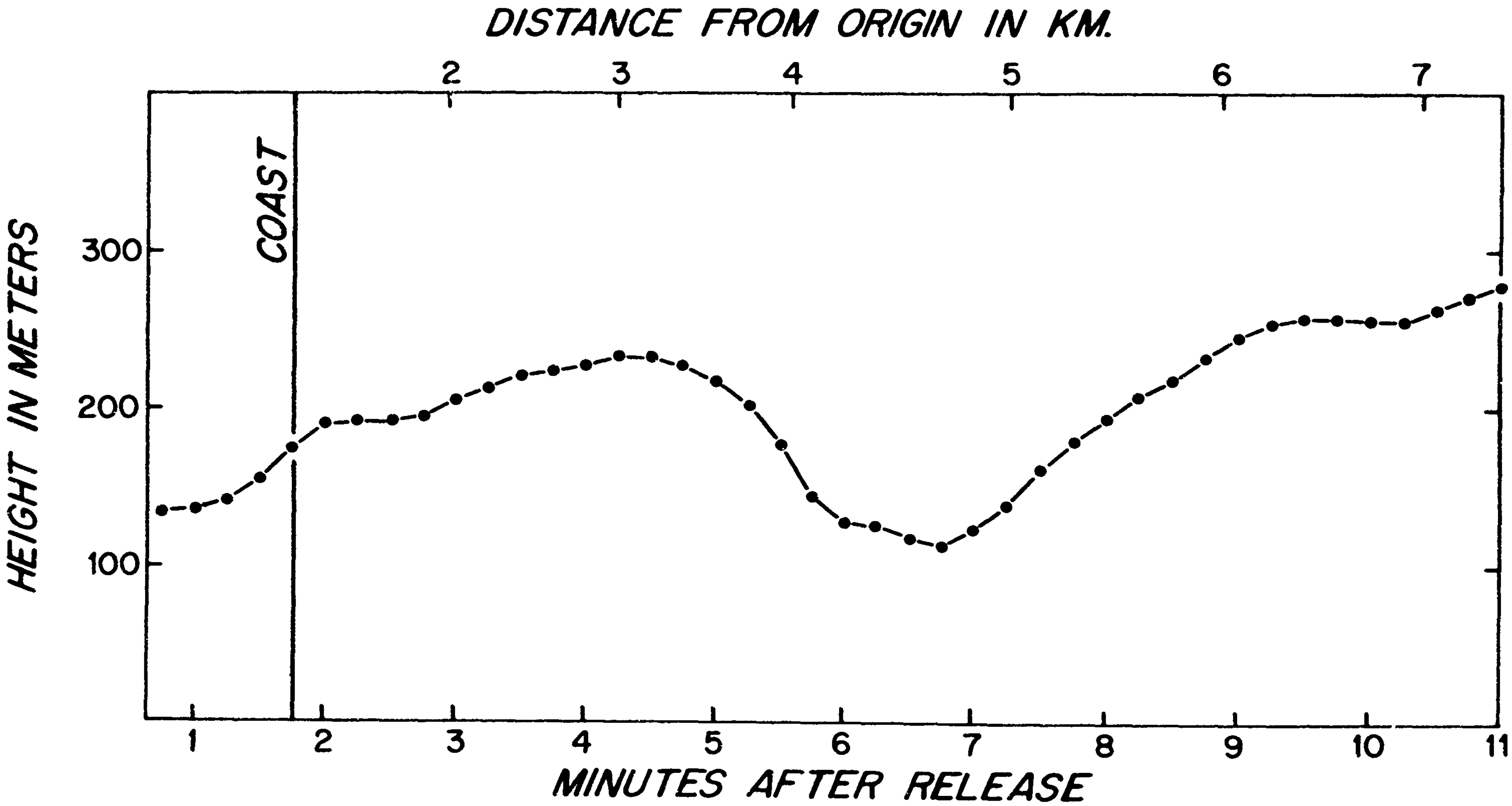
The effect was studied during 35 pilot balloon ascents which were observed with two theodolites. The two theodolite observation stations were on the western (leeward) beach of Aruba. The beach is sickle-shaped. A master station was established on the roof of a beach hotel. The second station was again on the coast, almost due north (bearing $353^{o}42'55''$) at a distance of 3294.15 meters. The base line runs entirely over water. The release point of the balloons was about three kilometers east of the center of the base line.

An effort was made to keep the balloons at a constant height. This was done in the usual way by balancing one balloon and by carrying it up with a second one. After a predetermined time, corresponding to a height of 200 and 400 meters in the present case, the connection between these two balloons was broken by the burning of a fuse. Dewey and Almy Darex 30 gm. pilot balloons were used for the experiment. Better results might have been obtained by superpressure balloons as described by Booker and Cooper (1965). The balloon experiment in the present case was only a sideline of the general work carried out in Aruba, and no attempt has been made to make any special preparations beyond a careful survey of the baseline and bearings.

Any mean rise or fall of the balloons was eliminated from the data before waves were actually studied.

A plot of one particular ascent is shown in the diagram. The observations generally suggest the existence of waves, whose crests and trouths remain at about the same distance from the coast between successive ascents. The waves develop most strongly at an approximative distance of 3 kilometers from the coast. Altitude and number is largest about noon when the Väisälä frequency is highest. It is least during the morning and evening observations. This conforms to Scorer's concepts. There is also some indication that longer waves become more prominent at the higher elevations. The data are not sufficient, however, to state this conclusion with confidence.

From an aviation point of view, the waves are associated sometimes with downward velocities in excess of 2 meters per second close to the sea surface. This would be uncomfortable for small aircraft. They also seem to be related to a bank of maximum whitecap development in the sea. This was related tentatively, in earlier studies, to a peculiarity of the wave spectrum development. It seems now that it should rather be accounted for in terms of wind variations beyond the coast.



Balloon Ascent March 9 1615 hours

Ballons separated 40 seconds after release.

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J. Malkus & M. E. Stern, 1953: The Flow of a Stable Atmosphere over a Heated Island. J. of Met. 10, p.30.

R. S. Scorer, 1951: Gravity Waves in the Atmosphere. Archiv für Met., Geoph. und Biolcl., Vol. 4, p. 176.

DISCUSSION ON KRAUS'S PAPER

KRAUS: I would specifically like to ask Dr. Estoque a question. If U becomes small or if your --(not discernible)-- frequency is very large, then you can have very large wave numbers here. In other words you can get something that will give extremely short wave lengths. I wonder if that can be used to explain the fact that in Aruba you have no sea breeze. In other words, the wind velocity never reverses. You have changes in velocity, but never a reversal in direction. But what about this fact that you get these waves and the wavelength becomes increasingly shorter as this term increases? Could that explain the formation of that sharp front which suddenly moves backwards in the direction of the island itself?

ESTOQUE: Can I reply later, because I have a question also? I didn't exactly get the main part of the question. What you are asking is, I believe, the effect of the sea breeze?

KRAUS: During the day, assume it blew out here, you will get this disturbance here and the waves behind it are progressively short, and you normally have something like this so it becomes progressively shorter. I wonder under conditions of less wind velocity if you can reach a stage where friction --(not discernible)--. Under these conditions, can you get something like a sharp discontinuity or perturbation forming which may then move in the opposite direction? That could explain these waves which become progressively sharper in higher altitudes and possibly explain the formation of the discontinuity.

ESTOQUE: Even if you have no small or short waves, you will still have a front which will form in that position due to the sea breeze effect.

KRAUS: Why is it in the form of a front?

ESTOQUE: Well, the prevailing wind there would advect the heat toward the left part of the diagram there, but then there is a pressure gradient which develops because of the warm island and these two opposing processes will tend to sharpen this front. I guess this is the only explanation I can give you, but if you try to integrate the equations which are proper to this phenomena, you always get a sharp front like that.

ROSENTHAL: I don't think the question you are asking can be answered within the framework of the theory you have there. You have presented essentially a diagnostic theory, which says you give me a U and F and a wave number and I will tell you what W is. But, if you asked the question why does W change, and why does the wavelength of the W pattern change, then you have a stability problem to solve and this is essentially a steady state theory you are looking at. So, I think you need a new theory to answer your question, which is essentially what Dr. Estoque said, also.

ESTOQUE: When you presented the cross sections there, the W field and the V, the V is the component across the coast line, is that right?

KRAUS: --(Not discernible)-- In all of these slides which are prepared here, V is actually the velocity at right angles to the mean directional propagation

ESTOQUE: Did you observe any change along the arrow component, the arrow which you have on the blackboard?

KRAUS: No, one of the interesting parts of this was that there is no systematic change in the direction of the velocity, either with height or with distance from the island. This is essentially two dimensional. You would expect it behind the island and you get a systematic approach through the isobars, which we didn't know, but the direction of the wind remains, on the average, the same. This was perhaps what you would expect. These are the isobars, this is the island. Where the islands rock, the flow over the island will be something like this, and then it will become along the isobars as you get further away or higher up. If there is such a turning, it was small and did occur in the majority of the ascents but by no means say something like 25 or 30 hours.

ESTOQUE: There must be a change in speed though along the main direction, because you have the vertical motions.

KRAUS: Yes, there must be. You would expect, of course, a certain amount of convergence here. Now we do find that lower down but there is apparently a maximum horizontal speed vertically that goes down to the surface, and that's why you get this rough water here. It's very small, it's very small indeed, and there's a very little change in the vertical. One thing which is interesting, and which has practical importance, I think, is that the vertical speeds, which were measuring by balloons close to the surface within the lowest 100-200 meters, are quite often more than 2 meters per second and in one or two occasions more then four meters a second. I am sure that this would make it quite uncomfortable.

ESTIMATION OF PRECIPITATION PROBABILITIES

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ABSTRACT

The utility of the incomplete gamma distribution for fitting series of weekly, monthly, and seasonal precipitation totals is discussed. This model has highly desirable statistical properties and has been found to be a good approximation for these data.

1. INTRODUCTION

A major objective of the analysis of precipitation data should be the estimation of the probability of exceeding specified amounts falling in periods ranging from hours and days to weeks, months, and years.

A reliable estimate of precipitation probabilities for weeks or months is sometimes difficult to obtain. Unusual combinations of precipitation amounts, produced by the influence of convective- or synoptic-scale disturbances, may radically alter the shape of the probability curves derived from different samples of equal length at the same location. These sampling fluctuations are known to exist even when all samples come from the same statistical population.

The characteristics of this "true" population or distribution are usually unknown for most atmospheric variables, and they must be estimated from a sample--in most cases, a very small sample--which will almost always be in error.

The primary problem in the analysis is then to use methods that result in the minimization of the error of estimation. There are several ways to do this The simplest, of course, is to use a larger sample of data. This is not possible for the meteorologist or climatologist in most cases. A second way to reduce this error is to smooth out the "noise" or meaningless irregularities in the sample, by using statistical methods which provide the "best fit" for the available data.

2. STATISTICAL ESTIMATION

Several techniques may be used for estimating the parameters of a data sample. Two of the more common are the method of least squares and the method of moments. Fisher (1941) found that the various methods of estimation do not give equally good results; some estimates or statistics are more variable than others. The best statistics, of course, are those with the lowest variability.

A well-known example can be taken from a normal population: the mean or expected value could be estimated in samples of 10 by averaging the smallest and largest values, or by averaging all the values. The latter estimate, obviously, should be better since it uses all the information. The variability (measured by the variance) from sample to sample of size 10, has been shown to be twice as large when only the extreme values are averaged. The median--also an estimate of the mean--has a variance about one-third larger than the mean for similar sampling.

This simple illustration points out that we should always use the most efficient estimates available to us, particularly where we are faced with relatively few data.

3. "GOOD STATISTICS"

Fisher (1941, 1954) gives three qualifications of satisfactory statistics--consistency, efficiency, and sufficiency. He defined these in the following manner:

1. A small-sample statistic or estimator is consistent if it converges in probability to the population or parameter value. Expressed in symbols this is:

$$P(|T_n - \theta| < \xi) > 1 - \eta \quad ; \quad n > N \tag{1}$$

   where T_n is an estimate of the parameter θ based on sample size n; ξ and η are arbitrarily small quantities, and N is any integer. $T_n = \theta$ when it is calculated from the whole population.

2. A consistent estimate T_1 is more efficient than another consistent estimate T_2 if the variance of T_1 is less than the variance of T_2. Efficient estimates are the class of consistent estimates with the smallest possible variance. Fisher (1954) concluded as a result of his many years of work that an efficient statistic can in all cases be found by the method of maximum likelihood. The efficiency is defined as the ratio $v(\hat{T})/v(T)$ where $\hat{T}$ is the maximum likelihood estimate.

3. An estimate T is called sufficient if it includes all possible relevant information which the sample (of any size) contains. The sufficient statistics, when they exist are definitely superior to other efficient statistics. If T_1 and T_2 are different efficient estimates of the parameter θ and are not functionally related, the estimate T_1 is sufficient if the joint distribution of T_1 and T_2 has the form:

$$f = f_1(T_1, \theta) f_2(T_2 | T_1) \tag{2}$$

where f_1 is the frequency distribution of T_1 and f_2 is the distribution of T_2 given a sample value of T_1. When T_1 is known, the probability of any range of values for T_2 is the same for all parameters; therefore T_2 cannot supply any information on any parameter which is not already available from T_1. This highly desirable property, particularily in small samples, makes these optimum statistics.

Fisher (1941) developed the method of statistical estimation referred to above as maximum likelihood. The original mathematics was due to Gauss. The procedure consists of maximizing what Fisher called "likelihood" or the product of the frequency functions of a sample.

If $f(x;\beta,\gamma)$ is any frequency function, likelihood is defined as

$$M = \prod_1^n f(x_i;\beta,\gamma);\tag{3}$$

taking logarithms this becomes

$$L = \sum_1^n \log f(x_i;\beta,\gamma).\tag{4}$$

Differentiation partially with respect to β and γ and equating the results to zero gives the maximum likelihood equations:

$$\frac{\partial L}{\partial \beta} = 0 \quad ; \quad \frac{\partial L}{\partial \gamma} = 0 \quad .\tag{5}$$

The solutions of these equations produce the maximum likelihood estimates, $\hat{\beta}$ and $\hat{\gamma}$.

Mathematical difficulties arise in the solutions for many functions, but Fisher found that, in most cases, approximations to maximum likelihood solutions were also efficient, consistent, and if a sufficient estimate existed, also sufficient.

4. ESTIMATES FOR THE GAMMA DISTRIBUTION

Thom (1947, 1958) applied (4) to the two-parameter gamma frequency distribution equation -- a special case of the Pearson Type III distribution with locus parameter zero :

$$f(x) = \frac{1}{\beta^\gamma \Gamma(\gamma)} x^{\gamma-1} e^{-x/\beta} \quad ; \ \beta > 0,\ \gamma > 0,\tag{6}$$

where x is a random variable, Γ is the gamma function, and β and γ are the scale and shape parameters of the distribution. He obtained

$$L = -n\gamma \log\beta - n \log\Gamma(\gamma) + (\gamma-1)\Sigma \log x - \frac{1}{\beta}\Sigma x.\tag{7}$$

Differentiating partially as indicated above the maximum likelihood equations are

$$x/\hat{\beta} - \hat{\gamma} = 0, \quad \log \hat{\beta} + \frac{\partial \log \Gamma(\hat{\gamma})}{\partial \hat{\gamma}} - \frac{1}{n} \Sigma \log x = 0 \cdot \tag{8}$$

The second term of the second equation is the digamma function $\Psi(\hat{\gamma})$ so Thom took logarithms of the first equation and substituted for $\log \hat{\beta}$ in the second to get

$$\log \hat{\gamma} - \Psi(\hat{\gamma}) = -\frac{1}{n} \Sigma \log x + \log \bar{x} \tag{9}$$

This is implicit in $\hat{\gamma}$ but has been solved with difficulty. Thom developed an approximation to $\log \hat{\gamma} - \Psi(\hat{\gamma})$ following Nörlund (1924) who showed

$$\Psi(\gamma) = \log \gamma - \frac{1}{(2\gamma)} - \sum_{k=1}^{m} (-1)^{k-1} B_k / (2k\gamma^{2k}) + R_m \tag{10}$$

is an asymptotic expansion in which B_k are the Bernoulli numbers: $B_1 = 1/6$, $B_2 = 1/30$ etc. and R_m is the remainder after m terms. For $\gamma > 1$

$$|R_m| < B_{m+1} / (2m+2) \gamma^{2m+2} \tag{11}$$

With $m=1$ and $\gamma=1$, $|R_m| < 0.00833$ or under 1.5 percent of the value $\Psi(1) = -0.57722$ given in tables. The approximation increases in accuracy with γ. From (10) for $m=1$

$$\Psi(\gamma) = \log \gamma - 1/(2\gamma) - 1/(12\gamma^2) \quad \text{substituting} \tag{12}$$

in (9) we get $12(\log \bar{x} - \frac{1}{n} \Sigma \log x)\hat{\gamma}^2 - 6\hat{\gamma} - 1 = 0$. Simplifying (13)

by calling the expression in the parenthesis A, the pertinent root of the quadratic is:

$$\hat{\gamma} = \frac{1 + \sqrt{1 + 4A/3}}{4A} = g \tag{14}$$

This equation together with $\hat{\beta} = \bar{x}/\hat{g} = b$ gives the maximum likelihood estimates for the gamma distribution.

The necessary computations for application are simple: the sum of the natural logarithms of X and the natural logarithm of the mean of X are all the basic data required.

The necessary and sufficient conditions for a set of estimators to be jointly sufficient have been given by Koopman (1936):

$$\log f = \sum_{k=1}^{p} A_k X_k + B + Y \tag{15}$$

where A_k and B are functions of the parameters (β and γ) and X_k and Y are functions of X. Thom took logs of the gamma distribution equation, getting

$$\log f = -x/\beta + (\gamma - 1)\log x - \log \Gamma(\gamma) - \gamma \log \beta \tag{16}$$

This is the same form as (15) where $A_1 = -1/\beta$, $A_2 = \gamma - 1$, $X_1 = x$, $X_2 = \log x$, $Y = 0$, and $B = -(\log \Gamma(\gamma) + \gamma \log \beta)$ and indicates that b and g are jointly sufficient estimates. Thus, no other estimates of β and γ can give more information on these parameters so they are the optimum statistics. Fisher (1941) has shown also that $\bar{X}$ is a 100 percent efficient estimate of the population mean.

Thom (1958) compared the efficiency of the method of moment fitting with maximum likelihood by using the variance ratios. He found at

$$\gamma = 1 : \quad \beta_m = 51\%, \quad \gamma_m = 39\%$$
$$\gamma = 10 : \quad \beta_m = 89\%, \quad \gamma_m = 88\% .$$

Thus for γ's < 10 moments are unsatisfactory: for γ near 1 only 40 to 50 percent of the information in the sample is used for estimating the parameters. At γ near 10 the efficiencies approach satisfactory levels.

5. APPLICATIONS

Thom's approximations have been applied to several appropriate classes of problems in climatology where variables have a physical lower boundary of zero and no upper limit. Precipitation is perhaps the most important of these, and the most extensive applications have been to various rainfall data.

Barger and Thom (1949) found that the gamma distribution had the statistical flexibility necessary to fit rainfall data series from one week in duration (usually of the negative exponential type) to three, four, or more months in duration. These latter usually approach the normal distribution, although a few large totals show they are positively skewed.

Friedman and Janes (1957) discussed rather completely the gamma smoothing of empirical (observed) cumulative rainfall probability curves obtained by

$$F(x) = \frac{m}{n+1} \quad , \quad m = 1, 2, \cdots, n \quad , \tag{17}$$

where $F(x)$ is the chance that a rainfall amount (weekly, monthly, or seasonal) is less than X inches but greater than zero. They found that the method provided a consistent basis for smoothing in New England.

Precipitation records for many stations in the North Central and Northeastern United States have been analyzed using gamma methods during the past few years. Probabilities of various weekly precipitation levels have been published (1960, 1961) under the guidance of the NC-26 and

NE-35 Regional Technical Committees -- cooperative groups of the State Agricultural Experiment Stations, the Department of Agriculture, and the Department of Commerce.

Kotz and Neumann (1963) in Israel have used the gamma distribution as the basis for obtaining estimates of precipitation probabilities for longer periods, given a short base period.

Several machine programs have been developed (Hartley and Lewish, 1959 a,b; Bark and Hofman, 1962) for fitting the gamma distribution to rainfall data and obtaining probabilities of specified precipitation amounts for various periods.

Recently, monthly precipitation probabilities have been obtained for about 200 stations in the southern and eastern United States. This was done in connection with work currently underway seeking to determine the effects of organized tropical cyclones on the distributions of precipitation over this region.

A few illustrations have been prepared to show some seasonal, monthly, and weekly cumulative probability distributions of precipitation. The period of record for all these data is 1931 through 1960, the current climatological normal period.

Figures 1 and 2 give two seasonal distributions at Apalachicola and Key West Florida, respectively. The periods chosen in each case the driest and wettest seasons. The irregular curves are the observed distributions, fitted by Eq. 17. The ordinate is probability, or the chances of receiving at least the amount of precipitation indicated on the abscissa. The "G" lines are fitted gamma distributions; the "N" lines are fitted normal distributions.

The usual tests for goodness of fit base on the χ^2 distribution are not applicable for these small samples. The maximum deviation test of Kolmogorov, given in Dixon and Massey (1951, p.450), indicates that both distributions provide "good" fits for both samples.

Figures 3 and 4 depict similar data for monthly periods at Apalachicola and Key West.

Figure 5 shows weekly distributions at Apalachicola. The gamma parameters, means, standard deviations and the maximum deviations are summarized in Table 1.

In all cases the deviations resulting from gamma fitting are smaller than from normal fitting. This type of comparison is admittedly not as rigorous as could be desired, but does indicate that the gamma distribution provides reasonable good estimates of precipitation probabilities for observed distributions with varying characteristics and different time scales.

Thom has shown (1958) the maximum likelihood parameter estimates are jointly sufficient, or optimum, estimates for the gamma distribution; and that this distribution is a good approximation to actual precipitation distributions because of its physical reality and statistical flexibility. This paper demonstrates that weekly, monthly and seasonal precipitation data from a subtropical region are also adequately represented by these gamma distribution estimates.

6. ACKNOWLEDGEMENTS

Thanks are due H.C.S. Thom who has provided information and advice.

Table I -- Rainfall distribution characteristics

APALACHICOLA

Period	g	b	$\bar{x}$	s	Max. Dev. g.	Max. Dev. n.
July-Sept.	12.51	1.93	24.19	7.18	13.7	14.7
Oct.-Dec.	4.93	1.62	7.98	3.74	8.6	11.5
September	3.74	1.95	7.31	5.94	9.8	18.2
October	1.71	1.88	3.21	2.61	10.0	17.0
Sept. 13-20	0.87	3.40	2.95 (29)	3.41	11.0	19.4
May 3-9	0.63	0.70	0.44 (20)	0.42	5.6	8.9

KEY WEST

Period	g	b	$\bar{x}$	s	Max. Dev. g.	Max. Dev. n.
July-Sept.	8.35	1.80	15.02	5.10	8.8	12.8
Jan.-Mar.	2.91	1.75	5.09	3.52	9.9	14.1
September	4.67	1.40	6.53	3.13	10.1	10.7
January	1.21	1.23	1.49	1.64	11.1	22.8

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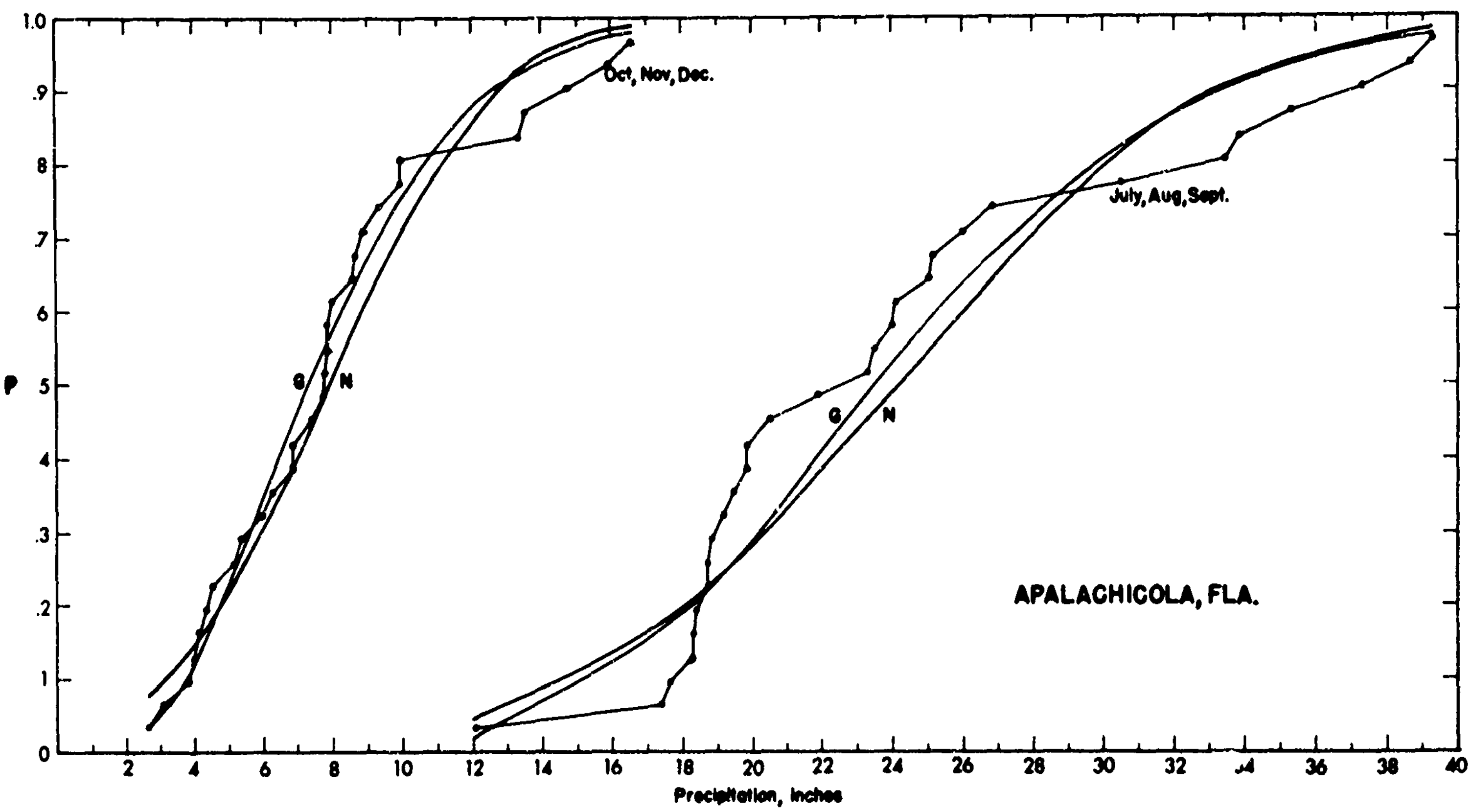

Figure 1 - Cumulative probability estimates of precipitation amounts for July-September and October-December at Apalachicola, Florida (1931-1960). Unsmoothed curve is actual rainfall, G is fitted gamma distribution, N is fitted normal distribution. P is probability of receiving less than indicated amount during the period.

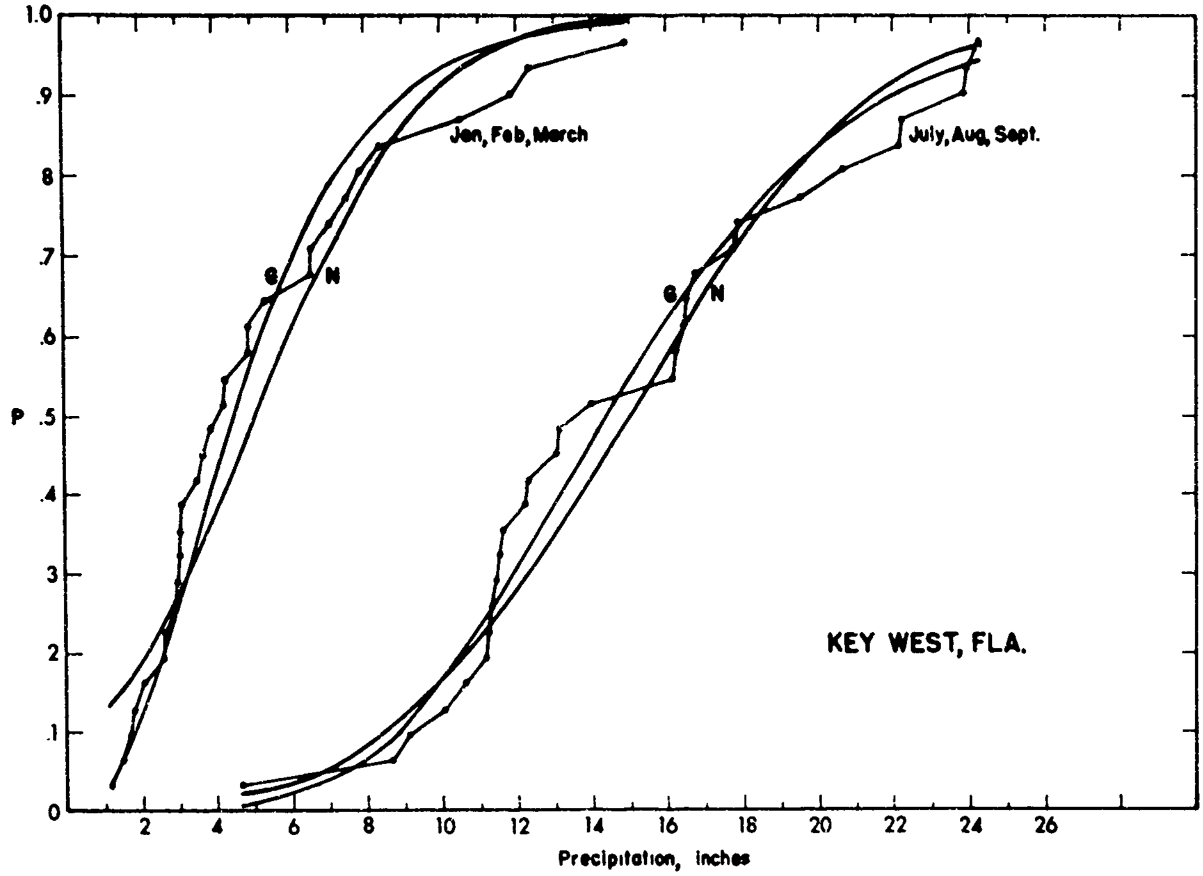

Figure 2 - Cumulative probability estimates of precipitation amounts for January-March and July-September at Key West, Florida (1931-1960). [Remainder as for Figure 1]

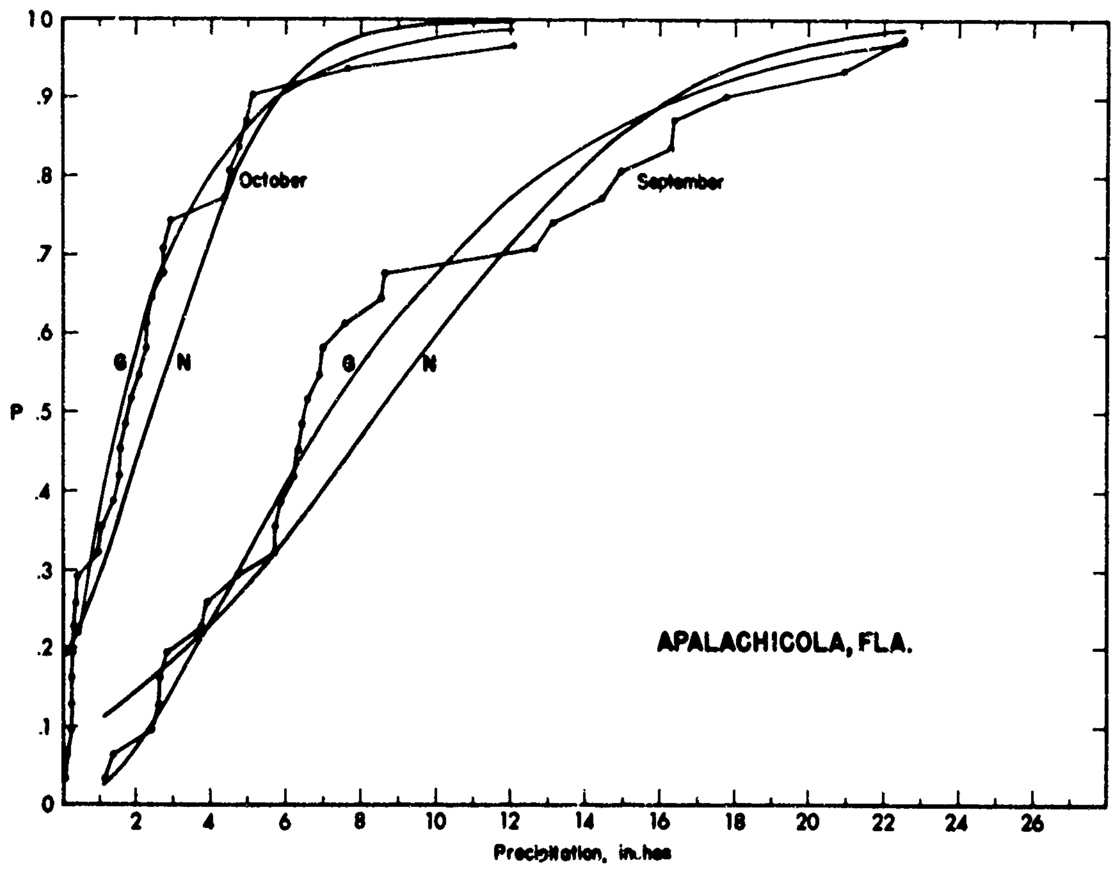

Figure 3 - Cumulative probability estimates of precipitation amounts for September and October at Apalachicola, Florida (1931-1960) [Remainder as for Figure 1]

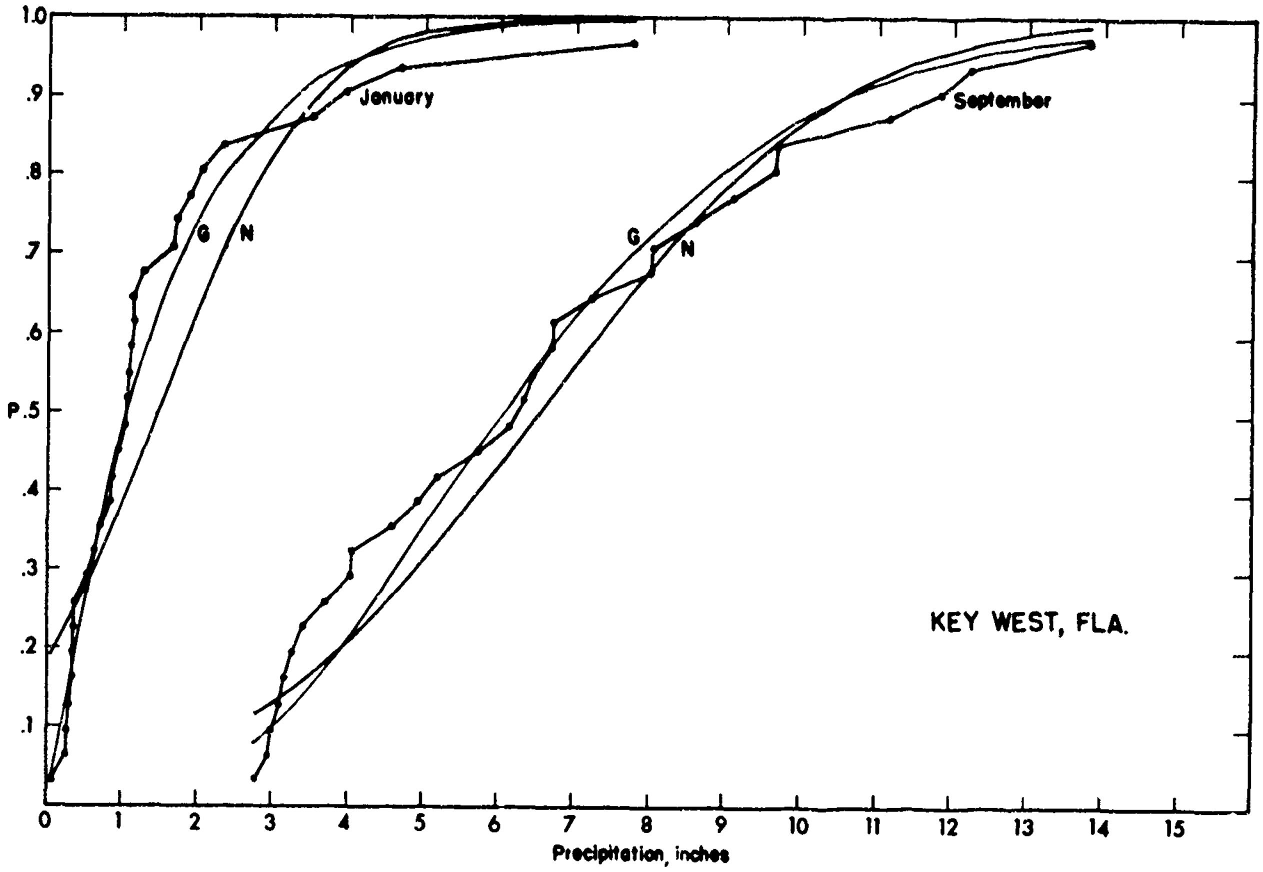

Figure 4 - Cumulative prob.bility estimates of precipitation amounts for January and September at Key West, Florida (1931-1960) [Remainder as for Figure 1]

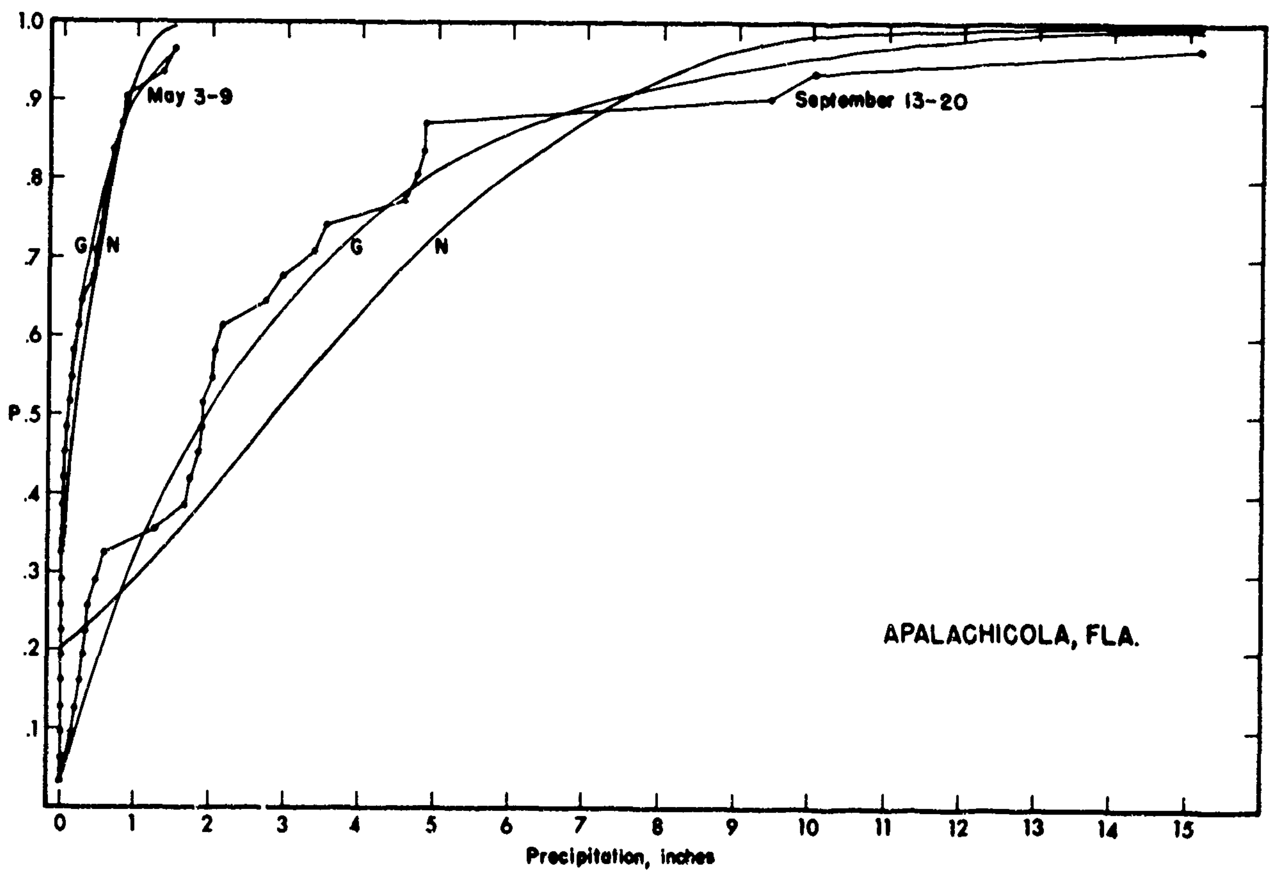

Figure 5 - Cumulative probability estimates of precipitation amounts for May 3-9 and September 13-20 at Apalachicola, Florida (1931-1960). [Remainder as for Figure 1]

DISCUSSION ON CRY'S PAPER

GRIFFITHS: Mr. Cry, I wondered whether you had tried any of the standard transformations to try to make your data into a normal pattern? I agree with you that with the flexibility of the incomplete gamma; that can take almost any shape dependent upon the time. This is for precipitation data lone. It has the advantage over the normal, but the disadvantage of no really good test, I mean -- (not discernible) -- as you say, is limited. I wondered whether you had tried to affect a transformation of a power form where the power is dependent upon the skewness of the distribution, providing the kurtosis is small, which we have found in the tropical patterns is usually the case.

CRY: No, sir, I have not, but I do want to look at some other distributions. Incidentally, I have been using 30 years of data here. I ran 60 years of data for some stations by hand and I found that the fits were much, much better. The other day when I showed this to Dr. Lansberg, he said, "Well, perhaps 30 years is not long enough to get us a reasonable normal, really." This is really based on one assumption. If we can say this is the best distribuiton, in other words, if this is the optimum distribution, and from the theoretical development, Fisher says that these estimates may be. Some of the internal things that Herb did, I don't know. Some of the approximations he made in here-this may not be the complete answer for all of them, of course.

AUTOMATIC PICTURE TRANSMISSION FROM SATELLITES

by

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Automatic picture transmission (APT) from satellites provides data particularly well-suited to users of these data who need coverage of their immediate area. This system is especially suited for meteorological support of the Army operations in remote areas. These pictures can be received directly from the satellite using comparatively simple equipment, thereby eliminating the need for communications with a weather central.

The purpose of this paper is to make two points in connection with satellite data research. The purpose is not to report research results. The points are these. First, the illustrations of APT pictures shown on the slides shows that the resolution and detail is equal that of the other TIROS pictures. For that reason many mesoscale system interpretations can be made. Especially over land areas the mechanism that influences the cloud pattern are several and complex. For that reason many of the research results already published cannot be generalized to these areas. Research into the interpretation of cloud patterns over tropical land areas have been neglected and now needs to be done.

The second point is to illustrate some of the suggested means of using APT data in support of Army operations.

The simplest use is, of course, the detection of an area of bad weather and make a forecast simply by extrapolating its motion along the wind flow. This is illustrated by the slide of the APT picture taken of typhoon Wilda off the Asiatic coast. It is clear that a day-to-day surveillance of a bad weather area like this would be extremely useful and might be the only information that an isolated Army unit might have. The storm could be tracked from day-to-day and a forecast made even though the unit had no support and communications from a weather central. This is a rather obvious example; there are more sophisticated uses.

Another example illustrated by slide of an APT picture is the cloud configuration that shows an on-shore and an off-shore wind regime. An illustration of a cloud pattern on the west coast of Borneo suggests a good sea breeze development. The main cloud line is slightly in-shore and a clear zone is off the coast, corresponding to the return sinking motion over the sea, figure 1.

Other examples of land produced patterns are shown by wave clouds downstream from mountains. These tend to be parallel to the ridges and at right angles to the mean motion of the air. On the other hand, another cloud pattern illustrated shows parallel lines which are along the mean flow. These are produced by mountain peaks setting off cumuli which then extend downstream in parallel rows.

The point of illustrating these various cloud patterns is to show that the interpretation of satellite data can yield important information about the temperature regime, the flow regime, etc. but the research already published cannot be generalized to these areas without examining the local effects. In other words, before these data can be fully exploited it is necessary to examine the patterns with standard data in similar geographical and climatological regimes. The suggestion is therefore that local climatology and local regimes be examined in connection with satellite data in order to provide a good basis for interpreting the APT pictures before they are needed for operations. I hope this Army program will sponsor such research. I hope that capable research organizations will be responsive to this need and submit their proposals to the appropriate offices. The primary Weather Bureau responsibility does not extend to these remote tropical land areas. While we are extremely interested in seeing this type of work carried out our support must first go to the research closer to home. For that reason it would seem that this program should be the prime responsibility of such organizations as the ones sponsoring this meeting. I would hope to see much new work along these lines started within the next year.

0415 GMT
AUG. 31, 1964
NIMBUS I APT
PASS 0042

DISCUSSION ON HUBERT'S PAPER

DUNN: Les, do you promise that the APT definition in the sate te to come will be as good as it was in Nimbus?

HUBERT: As good as in Nimbus APT?

DUNN: Yes

HUBERT: Well, he's asked me to promise that. I suppose he'll sue me if I'm wrong. I think that in general the answer is yes, but some of the APT from Nimbus taken from the perigee was better than all of the Tiros and better than the future APT, because the future APT should be at about a constant altitude of 450 miles. We were down to just under 300 miles with part of the Nimbus APT when it was taken at the low side and this gives you just about a linear proportional increase, in other words from 450 to 300, which is practically a third better definition on some of the Nimbus APT than we will have with future ---.

DUNN: Our reception from the Tiros that had the APT was very, very poor. Our reception from the Nimbus was excellent, and so we certainly hope that it is more like the Nimbus than the Tiros.

HUBERT: Yes, I think, Mr. Dunn, it's true that it will be more like the Nimbus, because there were other troubles, as you know, with the Tiros VIII APT, the scalloping, some of the electronics that they have cleaned up on the later one, so I'm sure it will be better. Our operation alone will be better than I think it was on the Tiros VIII APT.

LIST: When we start looking at data in remote areas, we always have to rely on the wonders of modern science, satellites, radar, and things like that. Is anything being done to see if we can correlate radar with satellite information, because it seems to me that these are really the two things that we will have to work with in some of the remote areas.

HUBERT: I see that we have a radar man who wants to answer that. Some work has been done by Ligda and his group at SRI, and I'll let Mr. Hiser speak for Miami.

HISER: Yes, we have been doing some of this at Miami and it is surprising how many cases there are when you have good satellite data and you are plagued by a lack of sufficient radar data to back it up or correlate with it. This has been a major problem in making the case studies that we have been trying to do, but aside from that, when we do have good data for correlation, it seems to me that interpreting the Tiros cloud patterns, particularly with the scale of resolution of the lenses that we have had up to this time, becomes almost as bad as the radar rainfall problem that radar people have been working on for years. It's very hard to pick out individual small showers or thunderstorms on the Tiros photographs to run correlations with what the radar sees. The radar in most cases is capable of seeing an order of magnitude smaller scale features than the Tiros can pick out at the present time with the present lens systems. We would like to see something done on this - better resolution.

<u>HUBERT</u>: I might add one comment. I mentioned that at SRI was doing some of this work and when they were faced with this problem, they had very much difficulty in making much sense between the correlations that are essentially between two scales. What they did was take a time integration of the radar pictures, which, of course, would smear it out, and then they found that there was a much better correlation between the time integration with radar and the patterns that appeared instantaneously on the satellite. This is sort of the direction you would expect it to go, and just how good these results are as far as being useful, I can't judge. But you are probably familiar with that, Homer.

<u>HISER</u>: Yes, we are familiar with the work that SRI has done, and I would say, too, that there is certainly, as Mr. List has pointed out here, a need for more work in this area of radar and satellite data correlation. In a couple of cases we have found what we think are definite misinterpretations of satellite data, of some of the early generations of satellites, Tiros series, due to the fact that conclusions were drawn without the use of radar support. We found one case off the west coast of Florida in which, what appeared to be a squall line evidently was one large thunderstorm with a huge blow off, which on the Tiros picture looked much like a squall line, but the radar to my satisfaction proved that it was not. This is only one isolated case, but it points up that, as Mr. List has said, there is definitely a need for this type of correlation.

<u>FREEMAN</u>: I would like to paraphrase and expand your pep talk to make sure I understand it. I don't mean the whole thing, but the punch line, and that is that you're recommending something of roughly this nature: that, when we see a pattern in a remote area from the satellite photograph, that we try to find an area with data that we feel is climatologically and geographically similar and study similar patterns in that area, so that we can begin to gather, make some quantitative estimate of other weather parameters other than the satellite information when we get a satellite picture from this remote area. Is that the kind of research?

<u>HUBERT</u>: Yes, that sums up my 15 minute paper in a sentence. However, I might say that while I'm enthusiastic about it, the type of support that the Weather Bureau itself gives out for research, is probably somewhat narrower than that and I was suggesting that this is the type of research that the Army and this group should be interested in pursuing to augment the Weather Bureau effort.

<u>GERRISH</u>: The Army has been working with this APT system for a number of years and perhaps Mr. Combs or Mrs. Whedon would like to comment on this. They've replaced the corkscrew with another type of antenna and a number of variations have been made for the mobile reception, mobile APT set-up. Would either of you Mr. Kulthau, or anyone, care to comment on that?

<u>KULTHAU</u>: The Army has in to Washington now a small development requirement for a mobile automatic picture taking ground receiving device. This will use a

single di-pole antenna, rather than a large corkscrew antenna, which was used with the initial equipment developed by NASA, as I recall. This is programmed to be a field item in 1967, if my memory serves me properly. We have come along quite nicely with it. We have a model of it up on our area now, which is open for anyone who wants to see it. I don't know what the readout is, Homer. Arnold Peterson at Fort Monmouth is a contact, if you are interested.

HUBERT: I might add just one little thirg. We have experimented with a very simple whip antenna. The overhead passes which get your immediate area can be readout quite well with this, and it's almost non-directional so you don't have to track. Any of the passes that are low over the horizon are not satisfactory with the whip antenna; you apparently have to have a higher gain antenna and track.

QUESTION: --(Not discernible)--

HUBERT: I'm not sure, I would be guessing, but I think it is of the order of 30-35 degrees, the noise starts getting so bad that you get essentially no signal. You have to have it at a zenith angle of 45 degrees, or something like that.

KUHLTHAU: I believe the one Mr. Peterson has developed can go down to 5 degrees with a fair degree of clarity.

HUBERT: Very well. Do you have to track?

KUHLTHAU: You do not have to track.

WHEDON: I believe the question was asked about what the readout was on the mobile unit. On the mobile model that has been built, they have used a Land camera photographic method. They do not contemplate at the present time remote indication, but they did tie in a tape recorder, so that if in the course of time they lost the data photographically, it is possible to play back and not lose the data along the way. I understand that when the first tests were made from the bird, NASA picked this up and requested that the other agencies also tie in a tape recorder, and we operated both a NASA type and a mobile type instrument. At this time, while they had a helical antenna, they had shortened it somewhat on account of the need for mobility, but they did use the whip antenna and got very successful and comparable results with the whip antenna.

GOLDMAN: I was wondering if the Weather Bureau had any plans on improving the rectification in any way.

HUBERT: The plans for improving the rectification are largely NASA's and that's put it into the orbit that's been predicted circular and triggered looking straight down so that its a very simple matter to do the rectification by just putting a grid on it. When there are variable heights and variable angles, then it is complicated, but they have every confidence that they are going to put it into just the type of orbit they've aimed at in the future.

COLDMAN: This is for small-scale stuff, too, I mean, I'm talking about tens of miles, not a couple of degrees, or anything like that. Is this what you are saying?

HUBERT: Well, the accuracy of a simple rectification scheme like this with landmarks that you can do the adjusting to, you can probably get accuracy within a few tens of miles, but if you are out over the ocean where there is nothing but the geometry and the predicted orbit to rectify to, then you are probably still talking about the order of one degree of latitude accuracy.

DUNN: --(Not discernible)--

HUBERT: Yes, Yes, that's true. In case the record didn't get that, Mr. Dunn made the point that you almost always have land to make accurate rectification with.

SENN. As some of you know we worked out a rectification system by means of reprojecting and then rephotographing. At the University of Chicago, Ted Fujita has worked out a much more complicated system, but one which is supposed to be more accurate. I'm not sure that he has actually brought it to completion, but in either case, this thing can be done by means of a computer in a matter of a minute or two per picture. I'm just wondering why, to get back to this gentleman's question, why we haven't gotten to this point, whether it's money, or what, on these higher elevation, or higher tilt angle picutres, or elevation changes. Why are we waiting until we get,(and this appears to be still over a year away,) pictures in nice circular orbits and looking essentially straight down at the earth when there seems to be a pretty good need for it at the present time?

HUBERT: Well, in connection with APT, of course, the idea is that every person who has $20,000.00 for an APT receiver, will not necessarily have the computer at hand, and so they are trying to emphasize the simplicity at some expense of accuracy. In connection with the regular world-wide Tiros operational things, the type of rectification, and we hope more accurate rectification that's being done, is being done with the computer, and not with the optical system just because it will be faster. I think the people who need extreme accuracy, like the work that you have done and what Dr. Fujita is working on, is a degree of accuracy and sophistication and complication and expense that the normal operational user doesn't want to pay. It is indeed necessary and so it's a time consuming and a research sort of thing to do in a laboratory, but not operationally.

SENN Well, I was thinking more in terms of either NASA or the Weather Bureau at the readout station, rather than having all of this stuff on tape ungridded, to have it done in one place and where you are retransmitting that to various users.

HUBERT Yes, this is the sort of thing that is --- already an experiment has been successfully run. We are bringing the raw signals from the readout station to Suitland, digitized and fed directly into a computer for rectification and this sort of thing. So I think we are moving in the direction you suggest. Things never go along quite as rapidly as we would like to see.

FREEMAN: I would like to make one comment along those lines, and that is that apparently it is not economical to grid past data, because from the standpoint of nearly every satellite picture user, he just wants n satellite pictures of a certain synoptic situation of a certain type, so that it's going to be cheaper and quicker to get those by these new methods that are being developed by fixing the orbit of the satellite, etc. than it is to go back to past data and do something sophisticated from the standpoint of gridding or something sophisticated from the standpoint of working better data out of an inherently worse picture that the ones you're going to get in the future. The only exception is that if you are studying some phenomenon like a hurricane, a tornado, or some solar phenomenon, like in our case the Mariner space probe flight, then you are stuck with the problem of doing the gridding in some way because no one else cares about that particular time.

HUBERT: Very correct, Dr. Freeman.

DUNN: Les, will these APT pictures still be about a thousand miles square?

HUBERT: Just about, possibly just a little under, maybe 700-800 miles when they are looking straight down across the middle.

DUNN: And if you obtain five pictures with, say, three consecutive passes, how much overlapping would there be?

HUBERT: The way the APT is now planned and is going to be programmed, the pictures are taken to just barely obtain overlap along the orbit, and at the latitude of Miami, there will be just barely overlap from adjacent passes too.

DUNN: Therefore, with five pictures, each a thousand miles square, very little overlap, you would be able to cover a very considerable area.

HUBERT: Yes, as you probably saw, some of the APT pictures that we read out in Washington, we got out almost to the central Atlantic and we got just about to the west coast, because you can read them low on both sides of your location.

DEVELOPMENT OF A TROPICAL ANALYSIS CENTER AT MIAMI

G. DUNN
NHC, MIAMI, FLORIDA

As all of you know, the World Weather Watch is in process of development. I think they now call the National Meteorological Center in Washington one of the two principal centers in connection with the World Weather Watch. In connection with that, of course, it is also planned to have regional centers, and some regional centers in the tropics, and probably we can say that we already have one or two.

Until it becomes possible to establish such a center somewhere in the American tropics, the U. S. Weather Bureau plans for the National Hurricane Center at Miami to undertake a regional tropical analysis. Starting last October 15th, we began to try to make analyses twice a day at the surface, and at 700 and at 200 millibars for the area from 130 degrees west longitude to about 40 degrees east and from about 40 degrees north to 35 degrees south of the equator. Now there are very large portions of this area from which we receive little or no data. During the last two months, a great deal of progress has been made in receiving data from South America, through the new circuit which has been established between Brazilia and Washington. The Dakar broadcast is copied at San Juan and transmitted to us. However, at one of these periods, 00Z, during certain periods of the year, of which this is one, receiptio of the Dakar broadcast is very poor. It is very good at 12Z and possibly might be at either 06 or 18. We will look into that.

It is planned that by the first of January, that by one means or another, we will be receiving most of the data from Africa, which will then give us two fairly good data areas on each side of the map, that is, North and South America on one side, Africa on the other. Now, particularly with Tiros IX and when both cameras were operating, once a day we got through the nephanalyses transmitted from Washington, a good picture of the cloud systems in the tropical areas and on the basis of that the ITC, for example, could be located quite accurately. Any tropical disturbances could be located quite well, and also in the Southern Hemisphere over the oceanic areas cold fronts could be located in pretty good shape. However, at the present time, one of the cameras is not in operation, so you get alternating strips, but through continuity and extrapolation, you can locate the ITC quite well.

Beginning May 10, we are planning on the new high altitude circuit, that is, the southern leg, to begin transmitting analyses at the surface and at 700 and 500 mb for a certain portion of this area, roughly from 130 degrees west to 55 degrees west and from 40 south to the equator. There are four surface analyses a day, two upper-air analyses at 500 and at 700 mb. Although we don't consider that our analyses in the tropics at the present time are much good. Certainly, I think that we can locate the ITC fairly accurately. I think that we are going to be able to pick up most of the disturbances on the ITC, but there are other weather producing processes for which we have no models. We see weather going on for which we do not know the reason. In part this is due to the fact that out over the oceans we have no upper-air data and the upper-air data that we get from the African and South American continents at the present time leaves much to be desired.

We are gradually obtaining additional inflight and postflight reports from high altitude aircraft. Most of the trans-oceanic flying is done at high altitudes, which gives us quite a bit of information at 200 millibars, but very little at 700 millibars, which we would certainly like to have. We have come up with, or we've seen some interesting things, things which have surprised us. Perhaps they shouldn't have surprised us. For example, I think it was just two or three days ago that, by chance, I think that we got about every raob in South America, which is taken regularly, and an unusual amount of high altitude flight information, that is winds, etc., more so than we have gotten on any other one date. Surprisingly enough from 40 degrees north of the equator to 35 degrees south there was not one single easterly wind reported. That does not necessarily mean that within 5 degrees of the equator, where certainly the winds were very light and variable (this is at 200 millibars), that there was not wind with some easterly component, but of the very considerable number of observations that we had, there was not one single easterly wind.

Well, we have not been given any assistance, personnelwise so far in connection with this new responsibility. It is being carried out by the aviation and the district forecasters over and above the work that they were carrying on previously. I know that it is the intention of the central office to give us additional assistance. They have certainly been working hard to improve the communications and I think that data-wise within a year or so, we will have perhaps not all that we need, but in one way or another, they will get to us about everything that is available.

Again, our forecasters and analysts have had very little experience, say in tropical South America, or in tropical Africa. We hope before a year from now, that in one way or another, we will have gotten some of our people, both to Africa and to perhaps Brazil, so that they will know more about analyses in these areas, and we are looking on this as something that is evolutionary. We think it will be a number of years before we will do the quality of work that we want to do, which I know you will want us to do. We hope that Dr. Rosenthal is going to come up with some assistance for us, certainly within 3 to 5 years. Perhaps a considerable portion of the forecasting, perhaps some of the analyses within that time will be done by machine methods, but at the moment, this is the status of at least our weak and feeble start.

DISCUSSION ON DUNN'S PAPER

HISER: Mr. Dunn, I have a comment regarding this new program and you may have some answers to it. One thing that has concerned us at the University of Miami since you have started this, is the requirement that has been placed on you causing you to have to change scale of your surface chart and as a result, I believe that you have had to leave out some of the data that has been plotted on these basic surface charts for the Gulf and Caribbean region in past years. We have long considered these Miami maps the best archival source for our research purposes, because it is a terrible job to dig up all the circuits from the archives in Asheville and replot the material, so we consider it important to have every piece of data available on the Miami surface charts. I wonder if there is a solution-perhaps the increase in the number of plotters available, or something else can be done in the future, so that there will be all of the data on the surface charts?

DUNN: During the winter season and so far, all of the data, as far as I know, have been plotted except from some stations in the continental United States. That is, the density of surface observations within the continental United States is such that we cannot plot all of them even in Florida. But I think all of the ship reports, and as far as I know, I may be wrong, but as far as I know, all of the data available, say from the Caribbean-Central American and extreme northern South America, have been plotted.

When the hurricane season arrives, which will not be in the too distant future, the first of June, there will be an increase in the number of ship reports, and there is certainly a possibility that a number of ship reports will not be entered. Now, our aviation forecasters have complained, to some extent, because of the smaller scale charts that we are using. I know that on the facsimile charts which we have transmitted on the test basis during the past few days of this week, that the scale is such that the data on the charts are not very readable, because they have to plot small to get it in, but at the present time we do not have the personnel to do any duplicate plotting. I think that if we ever do, that to make everyone happy, that at least for a small area, say the Caribbean-Central America, etc., that we will have to plot two charts, one on a somewhat larger scale than the one that we are now using. But the scale that we are now using is the same scale that is being used at Honolulu, the same scale that will be used in San Juan, and is the same scale that NMC is using in the transmission of the nethanalyses from the satellite, which makes it very easy, of course, to copy the nethanalyses directly on our surface charts, which we find very useful.

CLOSING COMMENTS OF THE LAST SESSION

FREEMAN: I can't say that I haven't said anything, but I would like to report on one piece of research that we're doing at our installation under NASA sponsorship, that ended up with us thinking about the 26 month cycle in the winds at the equator. We did what amounts to a periodogram analysis of the frequency of change of the number of hurricanes that occurred in the North Atlantic with the alteration of the 13 month winds, taking the time that the winds reached the stratosphere at one of these equatorial stations, Canton Island it was, at the time the winds changed at the stratosphere at Canton Island was roughly a 26-month period. We got in phase with that period and made a study of the frequency of hurricanes. When you do that with an event that occurs once a year, the frequency of hurricanes occurs only once a year, you have to use a special method of analysis, which was in this case to analyze 13 years data and get the wave that would result between 26 months and the once a year that you had. We found by that method of analysis and in phase with those winds, that there was a 26 1/4 month period in hurricane occurrences in the North Atlantic.

DUNN: Hurricane occurrence in the Atlantic, or world wide?

FREEMAN: No, just what we call hurricanes over here - up to Daisy, or Hilda, etc.

DUNN: What's your forecast for this coming year?

FREEMAN: We haven't made one, but we will make one and send it to you, and you can use it as you see fit. We might even send you enough copies that the paper will be useful.

DUNN: By when?

FREEMAN: Mr. Goldman will have Mr. Hannah send it to you when he gets back, so you will have it by about Wednesday.

DUNN: O.K., very good.

QUESTION: --(Not discernible) --

FREEMAN: This is in the same category as regards our research with NASA as some of the other things I've said today. It is something that we cannot justifiably work on and charge to some contract, and it's actually easier to get a new contract than it is to get our company to give us some money to work on without sponsorship. We have gone about as far as we can with this until someone else gets interested in it some time, so we have not worked with typhoons or Indian cyclones. We just happen to have worked with hurricanes.

FRISBY: I should just like to make one comment with regard to the sentences that Dr. Freeman has just uttered. That is that since this roughly 26-month cycle varies from cycle to cycle from something like 19 to about 29 months, I wish he would be good enough to tell us what length of cycle we are now in.

FREEMAN: I don't know. We don't have a current set of measurements on the winds. We're not keeping that record ourselves.

GOLDMAN: Miss Frisby, if you will contact Dr. Angel, his group is doing an awful lot of work with this and they're doing lots of calculating. When I was in Washington, I saw what they were doing and there is just mounds and mounds of it. I think they could tell you just exactly where we are now. I know that they can tell you where we have been within the last, I guess, 30 or 40 years.

SUMMARY AND CONCLUDING REMARKS INCLUDING NEW RESEARCH REQUIREMENTS

by
A. Combs
USAEL, Ft. Monmouth, New Jersey

I seem to be the representative left from the Army. Mrs. Whedon had to catch a plane and Dr. Weickmann, as you know, left for California this morning. I'm sure he would have liked to have said something at this time. Briefly, what I would like to say is this. I thank all of you for coming and participating. I assure you that Marvin Lowenthal is sitting back there in his wheelchair thinking of you. He has the agenda before him and I know he would have liked to have been here. Regarding this Conference, I think you appreciate the difficulty of preparing the Proceedings, writing them up, and translating the tapes of the discussions. We ask for your cooperation. Mr. Gerrish has a list of all those who have already given papers to him or Mr. Hiser. There are those who for one reason or another wish to withhold them for a short while. Please get these papers in so that the Proceedings can be prepared as soon as possible for maximum use.

I have become involved in this Conference primarily because of Marvin's illness. In discussing this with him on Tuesday, he left me with two thoughts to pass on to you. First, this is basic research. It is not the responsibility of our Army contract people to get involved in operational applications, although much of the work will certainly be useful in this respect. Second, it is important for you who are conducting research for the Army and others in this field to meet occasionally in order to thoroughly discuss contractual work and to share ideas about future work in tropical meteorology.

In conclusion I would like to express thanks on behalf of USAEL to Mr. Hiser and Mr. Gerrish, for I think they have done a fine job in setting up this program. Mrs. Snape, the convention manager here, has done a tremendous job in adding to the success of our Conference. We are pleased to have several guests from the National Hurricane Research Laboratory and the National Hurricane Center of the Weather Bureau in Miami. Miami offers an unusual setting for such a Conference in that a number of tropical experts, both U. S. and foreign, are working or visiting here. We appreciate your contributions to the Conference.

If there are any suggestions or comments on this Conference, don't hesitate to let me, Marvin, Dr. Weickmann, Harold or Homer know about it. It's been a most rewarding Conference and we hope to see all of you next year. Thank you.

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13. ABSTRACT

The primary interest of the meeting was in the realm of local, small scale meteorological phenomena and problems in the tropics, specifically excluding hurricanes and typhoons, although the papers were not completely limited to those topics. One objective was to learn as much as possible about the recent results and future research plans in tropical meteorology of the U.S. Army contractors as well as other invited guests from the United States and abroad. Nineteen papers were presented at the two-day meeting. Discussions of the papers were recorded and are included in the Proceedings.

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